Impacts of the Project NetWork Demonstration

Final Report

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Executive Summary

The Social Security Administration (SSA) initiated Project NetWork in 1991 to test case management and referral approaches to providing rehabilitation and employment services to promote employment among beneficiaries of Social Security Disability Insurance (SSDI) and applicants for and recipients of Supplemental Security Income (SSI) for blind and disabled individuals. To allow rigorous evaluation, eligible persons who volunteered for the demonstration were randomly assigned to either a treatment group eligible to receive the case/referral management services provided by the demonstration, or a control group who did not receive these services.[1] To increase the incentive to work, volunteers in both the treatment and control groups were also offered waivers of specific SSDI and SSI program rules considered to act as work disincentives. This report presents the results of the impact and benefit-cost analyses of the demonstration. The impact study analyzes the effect of Project NetWork services on earnings, receipt of SSI and SSDI benefits, and measures of health- and well-being. The benefit-cost study compares benefits and costs of these services from the standpoint of volunteers, federal and state governments, and society as a whole.

This report is one of four reports on Project NetWork produced by Abt Associates. In 1992, Abt Associates was awarded a contract to evaluate the effects of Project NetWork. The findings from the implementation study were presented in a 1996 report.[2] A second report[3] analyzes the decision of eligible persons to volunteer for Project NetWork services. A third report[4] presents the findings of a non-experimental analysis of the effects of the demonstration waivers. Together, these reports provide the first rigorous study of the effects of providing vocational rehabilitation (VR) assistance to persons with severe disabilities.

Encouraging people with disabilities to work by removing the barriers to seeking and retaining employment is a high-priority policy issue. Although persons with disabilities face special challenges in finding employment, recent advances in technology and medical treatment, and the passage of the Americans with Disabilities Act of 1990, have improved their opportunities for success in the labor force. Despite these advances, evidence indicates that little success has been achieved in rehabilitating and encouraging return to work among participants in SSDI and SSI, the two largest federal programs serving people with disabilities. As part of the budget for Fiscal Year 2000, the Clinton Administration is proposing new initiatives to help people with disabilities

1 The Project NetWork demonstration was designed as a randomized field experiment through the collaborative efforts of the Office of Disability at SSA and the Office of Assistant Secretary for Planning and Evaluation (ASPE) at the U.S. Department of Health and Human Services (HHS).

2 Wood *et al* (1996).

3 Burstein *et al* (1999).

4 Burstein *et al*. (1999),

retain employment. These initiatives would allow SSDI beneficiaries and SSI recipients to retain eligibility for Medicare and Medicaid for longer periods when they return to work, create a new incentive-based system for funding return-to-work services by private and public service providers, provide tax credits for work-related expenses, and fund the development of new information and communications technologies for persons with disabilities.[5] Given the current policy debate, it is critical to provide reliable information on the effectiveness of return-to-work programs combined with increased work incentives.

Demonstration Design

Project NetWork tested four models for providing employment and rehabilitation services. Each of the four models, distinguished by different institutional settings and staffing arrangements, was operated for 24 months in two sites during the early to mid-1990s:

- Model 1, the SSA Case Manager Model (Dallas and Fort Worth), featured the provision of case management services by SSA staff.

- Model 2, the Private Contractor Model (Phoenix/Las Vegas and Minneapolis), offered case management services delivered by private rehabilitation organizations under contract to SSA.

- Model 3, the VR Outstationing Model (New Hampshire and Richmond), featured the provision of case management services by State Vocational Rehabilitation Agencies, with case managers "out-stationed" in local SSA offices.

- Model 4, the SSA Referral Manager Model (Tampa/Carrollwood and Spokane/Coeur d'Alene), offered the least intensive case and referral management service, provided by SSA staff. Referral managers were to locate case management and other services for clients by accessing existing service providers in the community.

Participation in Project NetWork was voluntary. Members of the target population -- all SSDI beneficiaries and SSI applicants and recipients living in the service areas of all participating field offices -- were eligible to participate regardless of age, type or severity of disability, or other factors used in traditional vocational rehabilitation programs to screen out individuals judged not to be promising candidates for rehabilitation. Individuals who responded to demonstration outreach met with local demonstration staff and were provided a detailed explanation of the demonstration, and the opportunity to volunteer. Those wishing to volunteer were then randomly assigned to the treatment group or to the control group. Those assigned to the treatment group met individually with a case or referral manager who arranged for necessary assessments,

5 It is important to recognize that the Project NetWork demonstration is a test of case-managed return-to-work services and is not a test of the effectiveness of these new initiatives.

developed an individual employment plan, and identified and arranged for rehabilitation and employment services needed to achieve the plan. Of the roughly 145,000 persons who were solicited for the demonstration, 8,248 volunteered and were randomly assigned.

The impact study measures only the "incremental effects" of case and referral management services. Volunteers assigned to the control group remained eligible for any employment assistance already available in their communities. The difference between the treatment group and control group, then, is the addition of SSA-sponsored case/referral management services for the randomly selected treatment group. For *both* treatment and control group members, the demonstration waived specific SSDI and SSI program rules considered to act as work disincentives, so the experimental impact analysis cannot isolate the effect of these waivers. The impact study thus estimates the incremental impacts of the case/referral management services, above and beyond the effects of the waivers and whatever services the same individuals would have received absent the demonstration.

The impact study estimates the effect of Project NetWork services on a range of outcomes. For the case and referral management services of Project NetWork to be effective, they must increase the rate at which volunteers receive services or the quality of these services. The services include assessments, psychological counseling, physical and occupational therapy, job search assistance, job training, and other employment-related services. We first ask:

- Does case/referral management increase the percentage of participants receiving employment, training, and rehabilitation services?

These demonstration services were intended to increase participants' earnings, either *directly* as a result of the employment-related services, such as job placement, job search activity, and/or vocational skills training; or *indirectly* through improvements in health status or attitudes about working. A key question is:

- Does case/referral management increase the work effort of project participants, as measured by earnings, employment, months and hours worked, or earnings per hour? Do these services increase the percentage of participants receiving critical employer-provided benefits?

An important consequence of earnings gains would be to reduce participants' transfer income, that is, income derived from SSI, SSDI, and other sources of assistance. Health and psychological outlook could also change due to the demonstration, presumably in a positive direction. Accordingly, we also ask:

- Do these services reduce receipt of SSI and SSDI benefits? By how much?

Finally, we compare the benefits of Project NetWork and the costs of providing these services:

In these comparisons, we use the estimates of impact on benefit receipt, earnings, and other outcomes. In addition, a full analysis of costs and benefits depends on assumptions about how impacts on benefits and costs observed over the 2-3 year follow-up period project into future years.

- Do the benefits of Project NetWork exceed demonstration costs, producing net benefits to participants, the federal government, state governments, and/or society as a whole?

We examined these questions for the entire sample of persons randomly assigned, as well as for subgroups defined by title of eligibility at random assignment (SSI, SSDI, concurrent recipients, or recipients of neither benefit) and primary impairment, to determine whether Project NetWork services were especially effective for certain subsets of the overall sample. The evaluation uses information collected from SSA administrative records data, two in-person surveys of demonstration treatment and control group members, and automated demonstration records.[6]

Findings for the Full Sample of Project NetWork Volunteers

Project NetWork increased the percentage of persons reporting receipt of employment, training, and rehabilitation services by a statistically significant 6 percentage points. According to responses to the follow-up survey of participants, about 75 percent of treatment group members and 69 percent of control group members received at least one type of service. Project NetWork increased the percentage of persons receiving job search assistance from 14 to 21 percent, increased the percentage of persons receiving a work-related assessment from 17 to 27 percent, and increased the percentage of persons receiving business skills training from 6 to 11 percent, with smaller increases for other services.

Project NetWork increased average annual earnings by $220 per year over the first two years following random assignment. This statistically significant impact, a roughly 11 percent increase in earnings, is based on administrative data on earnings. Because random assignment occurred over two years and we have earnings data for calendar years through 1996, only about 70 percent of sample members have a third year of follow-up data. For this limited sample, the estimated effect of Project NetWork on annual earnings declined to roughly zero in the third follow-up year.[7] The overall impacts estimated from follow-up survey data were generally of the same magnitude, but were not statistically significant. The survey data also indicate that Project

6 In addition, impacts on earnings and disability benefits were estimated for subgroups defined by site and model. In general, there were few statistically significant differences in impact across sites and models, in part because of the small samples available at this level. Moreover, those differences that were found are difficult to interpret, because they may reflect nonprogramatic differences among the sites.

7 Earnings gains were found to be largest among the 30 percent of the sample that did not have a third year of follow-up data. There is some possibility, therefore, that earnings gains may have persisted, at least for some subgroups.

NetWork increased the total number of months of employment over the first two follow-up years, from 3.5 months to about 4.2 months, a statistically significant effect.

These findings suggest that Project NetWork's return-to-work services successfully increased earned income. However, the increase in earnings may have been short-lived, and may have disappeared at roughly the time Project NetWork services ended. The size of the average impact, $220 per year, was not enough to increase the living standards of the average demonstration volunteer by a meaningful amount. It is possible, however, that Project NetWork could have produced very large earnings gains for a small group of demonstration participants.

Project NetWork did not reduce reliance on SSI or SSDI benefits by statistically significant amounts. Over the 30 months following random assignment, the estimated impacts on the percentage of persons receiving SSI were well under one percentage point, and the estimated impact on average monthly SSI benefits was about $1. Over the 42 months after random assignment, the estimated impacts on the percentage of persons receiving SSDI were also well under one percentage point, and the estimated impact on average monthly SSDI benefits was about $3. While members of the treatment group indicated that they valued the services Project NetWork provided, the treatment group showed little or no measurable improvement in health or well-being relative to the control group.

The waiver provisions may have prevented Project NetWork services from reducing SSDI benefits for at least two years after random assignment. The waivers were intended to remove strong work disincentives in the SSDI program. These waivers were activated in the first month in which earnings exceeded $200 or self-employment exceeded 40 hours (the same criterion used to determine a Trial Work Period (TWP) month[8]). Once in effect, the waiver continued for the next 12 months regardless of subsequent employment. For SSDI beneficiaries in this waiver period, no month could be counted as part of the TWP, or result in benefit interruption for those who were in the extended period of eligibility. After the waiver period ended, earnings gains for this group will still not affect benefits for up to another year (the 9-month TWP plus the 3-month grace period). For those SSDI beneficiaries, then, increases in earnings would not result in benefit reductions until at least two years after random assignment.

On the other hand, the demonstration's impact on earnings may not have been large enough to cause a substantial impact on benefit receipt, even in the absence of the waivers. The estimated

8 The Trial Work Period (TWP), one of the standard work incentive provisions in the SSDI program was enacted as part of the Social Security Amendments of 1960 (P.L. 86-778). Each month in which earnings from work exceed $200 or self-employment exceeds 40 hours is counted as a TWP month. The TWP provision allows SSDI beneficiaries to have a total of nine such months during a rolling period covering the most recent five years. During the TWP benefits are unaffected by earnings. At the end of the TWP, a determination is made concerning the beneficiary's ability to sustain earnings at the substantial gainful activity (SGA) level. If earnings are lower than SGA levels (i.e., $500 per month), regular SSDI eligibility is continued. If earnings have consistently exceeded the level of SGA during the TWP, cash benefits are then continued during a three-month grace period, and the beneficiary simultaneously enters the 36-month Extended Period of Eligibility (EPE).

impact of the program on average annual earnings was $220. The waivers had no effect on the many participants whose earnings were not increased by the program. It seems likely that the program increased earnings for some persons by an amount which, in the absence of the waiver, was simply not large enough to have triggered a review or to have increased the number of TWP months. Therefore, for the participants whose earnings increased under the program by a small amount per month, the waiver may not have been needed to maintain eligibility for benefits. Another possibility is that demonstration services may have increased earnings by relatively large amounts among the small proportion of persons who had left SSI and SSDI.

The effect of the waivers on SSI recipients could be expected to be much less substantial. In SSI, the waivers prevented special disability or blindness reviews that could normally occur when earnings exceeded $500 per month. But these reviews were seldom conducted during the demonstration period in any case, and the waivers had no effect on the SSI "tax rate" on earnings, which immediately reduced benefits by $.50 for every $1.00 of earnings above $65 per month under the regular program rules.

In any case, these findings suggest that services like those provided by Project NetWork will not reduce overall SSI and SSDI caseloads or benefits by substantial amounts. This conclusion seems especially clear when we recall that only about 5 percent of the eligible SSI and SSDI caseload volunteered to participate in Project NetWork. The impacts of Project NetWork on benefit receipt of the entire SSI and SSDI caseload, many of whom may be too disabled to participate in a return-to-work program, are therefore far smaller than the impact estimates presented here.

In sum, the effect of Project NetWork on the full sample of volunteers was to increase earnings modestly without reducing average benefits. Even with the aid of the generous case and referral management services provided by Project NetWork, reducing reliance on SSI and SSDI benefits for these persons over a 30 to 42-month follow-up period is obviously a challenging task. The problems faced by persons with disabilities often last many years, if not a lifetime, and the typical spells of SSI and SSDI receipt are far longer than the follow-up period available for this study. The measures of health and well-being clearly show that substantial proportions of Project NetWork participants still face serious barriers to work.

The costs of service delivery were higher under Project NetWork. The average cost per treatment group member of services directly paid for by Project NetWork was $3,660 per person. The average cost per treatment group member of non-NetWork services (which may have been obtained by referrals but which were paid for by other services providers) was estimated to be $326. The total average cost of services per treatment group member is therefore estimated to be $3,986. The total average cost of services per control group member (consisting entirely of non-NetWork costs) is estimated to be $1,779. Thus, Project NetWork spent more dollars on service delivery than experienced by the control group, and the net incremental cost per treatment group member is estimated to be $2,207.

Project NetWork produced modest net benefits to persons with disabilities, and net costs to taxpayers; from a social perspective, costs probably exceeded benefits. Persons with disabilities gained mainly because the increases in their earnings easily outweighed the small reduction in average SSI and SSDI benefits. For the Social Security Administration and the federal government as a whole, the costs borne by Project NetWork were not sufficiently offset by increases in tax receipts from increased earnings, or from reductions in average SSI and SSDI benefits. Although state governments actually gained from Project NetWork, which displaced the states's costs of providing services through state-run VR agencies, all levels of government as a whole (and, thus, taxpayers as a whole) experienced costs in excess of benefits as a result of the demonstration. These findings remain essentially unchanged if we assume that the program's impacts on earnings continue but decay over the years immediately after the follow-up period.

Findings for Key Subgroups

In an effort to understand whether Project NetWork had larger effects on some persons in our sample, we estimated effects on earnings for several subgroups of interest. As we have stressed throughout this report, interpreting estimated impacts for subgroups requires caution. Whenever we analyze impacts for subgroups, the sample size declines, and the standard errors of estimate for many of the subgroups become quite large, so that only large impacts could be detected as statistically significant. Finding statistically insignificant impacts need not rule out the presence of smaller impacts. Similarly, statistical tests of the differences in impacts across subgroups are often a weak test of whether differences in impacts are present. At the same time, we have to be concerned about "false positives" with a large number of subgroup impact estimates because there is some chance that any given estimate will be statistically significant by chance alone, even when the true effect is zero. Finally, even when we do find statistically significant impacts for subgroups, that we believe are real effects, the interpretation of these findings is often unclear.

We found that Project NetWork reduced benefit receipt by statistically significant amounts among those receiving neither SSI nor SSDI at random assignment. Most of those receiving neither type of benefit at random assignment include SSI applicants whose applications had been denied or were still pending; for this group, the program also reduced benefit receipt by a statistically significant amount.[9] This subgroup had the weakest attachment to SSI or SSDI: even among control group members in this group, average monthly SSI and SSDI participation rates were under 10 percent. The estimated impacts on measures of benefit receipt for the other three title of eligibility subgroups (those receiving SSI only, SSDI only, or both at random assignment) were mostly much smaller and insignificantly different from zero.

We also found that estimated impacts on average earnings were statistically significant and largest for the subgroup who received SSDI only at random assignment. It is possible that Project

9 The rest were referred to the demonstration from other programs.

NetWork produces larger earnings gains for these persons because they have more work experience and perhaps need fewer services to return to work, and because the waiver provisions produce a stronger incentive to increase earnings for this group than for SSI only recipients. On the other hand, the estimated impacts on earnings for this group were not statistically significantly different from estimated impacts for those who received SSI only at random assignment or who were solicited as SSI applicants but did not receive benefits at random assignment. We therefore cannot conclude that the program had larger effects on earnings for SSDI beneficiaries than for SSI recipients.

When we categorized sample members by primary impairment, we found that estimated impacts on earnings were greater than zero and statistically significant for those whose primary impairment was other than mental, neurological, or musculoskeletal. However, estimated impacts for this group were not statistically significantly different from estimated impacts for those with mental or musculoskeletal impairments. The demonstration generally did not produce statistically significant impacts on measures of benefit receipt within these primary impairment groups. The only statistically significant effect over the follow-up period as a whole was among those with impairments affecting the musculoskeletal system, where Project NetWork reduced the percentage of persons receiving SSDI benefits by an average of 2.1 percentage points.

Recent SSA Initiatives

Project NetWork represented SSA's first large-scale involvement with private rehabilitation providers, and marked the first time that SSA provided services directly to its client population to help them enter or reenter the workforce. SSA's more recent efforts to increase the number of beneficiaries with disabilities who work represent a departure from direct service provision and the case management approaches tested under Project NetWork, focusing instead on providing greater incentives for public and private sector providers of employment and rehabilitation services and enhanced beneficiary choice. In 1997, SSA initiated the Alternate Participant (AP) program, the first substantial effort aimed at tapping the resources of the private rehabilitation service providers. The AP was implemented under existing legislative authority as an enhancement to the current VR referral program to expand the opportunities for beneficiaries to receive VR services.

Another major change to the current SSA VR program is the Ticket to Independence program, proposed by the Administration in 1997. Under this proposed program a beneficiary with a long-term impairment would be issued a ticket providing access to a broad range of employment and rehabilitation services. The tickets could be given to a provider chosen by the beneficiary in exchange for rehabilitation and employment services. The certified private and public providers who are offered and accept a ticket would be compensated only after the beneficiary is placed in a job and achieves independence from SSA's disability benefit rolls. The providers would be paid a portion of the benefits savings realized by SSA as a result of the beneficiary's work activity. It

is hoped that these new initiatives will remove barriers to work and increase the earnings of persons with disabilities on a cost-effective manner. [10]

10 In June 1998 the House of Representatives passed H.R. 3433, the "Ticket to Work and Self-Sufficiency Act of 1998" which included the Ticket to Work program and many of the elements of SSA's proposal. This ticket program has also been included in a Senate bill, S. 331, the "Work Incentives Improvement Act of 1999."

Chapter 1
Introduction

Productive, gainful employment has long been regarded as essential to the well-being of Americans and their families. Although persons with disabilities face special challenges in finding employment, recent advances in technology and medical treatment and the passage of the Americans with Disabilities Act of 1990 have improved their opportunities for success in the labor force. Despite these advances, little success has been achieved in rehabilitating and encouraging return to work among participants in the Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) programs, the two largest federal programs serving people with disabilities. Daniels and West (1998) report that the percentage of beneficiaries leaving the rolls for the purpose of returning to employment remains below 1 percent. In a 1992 study, Muller found that among a cohort of SSDI beneficiaries initially entitled for benefits in mid-1980-1981, only 2.8 percent had benefits terminated for reasons of employment. Of those, nearly one third were found to have returned to the rolls by 1990.

Interest in improving the employment prospects for persons with disabilities is keen. In March 1998, the President signed an executive order establishing a Presidential Task Force on Employment of Adults with Disabilities.[1] The task force is characterized both by its broad mandate and the number of high ranking administration officials who serve. The goals of the task force are to create a coordinated and aggressive national policy to address barriers to employment for adults with disabilities and to bring adults with disabilities into employment at a rate that is as close as possible to that of the general population. In carrying out its mission, the task force will examine existing programs and policies, develop options to address health insurance coverage, and analyze youth employment programs. One of the task force's early recommendations is that the administration continue working with the Congress to pass legislation that helps people with disabilities maintain health care coverage when they return to work. As part of the budget for the coming year, the Administration will propose new options that would allow SSDI beneficiaries and SSI recipients to retain Medicare and Medicaid benefits for a longer period when they return to work. In addition, the proposal calls for tax credits to help meet the costs of returning to work, and increased investments in assistive technology.[2]

Project NetWork is a demonstration initiative of the Social Security Administration (SSA), initiated in 1991 to test alternative methods of providing rehabilitation and employment services to SSDI beneficiaries and SSI disabled/blind recipients and applicants. The Project NetWork demonstration tested a case management approach to provide this population with rehabilitation

1 Presidential Task Force on Employment of Adults with Disabilities (1998).

2 Office of the Press Secretary, White House (January 13, 1999). The legislation currently being considered by the Senate is S. 331, "The Work Incentives Improvement Act of 1999."

and employment services and to encourage and facilitate their movement into the labor force. Having solicited more than 145,000 individuals for participation, Project NetWork is the largest return-to-work demonstration targeting SSDI beneficiaries and SSI recipients and applicants ever undertaken.

Project NetWork was initiated under the research and demonstration authority of Section 505(a) of the Social Security Amendments of 1980, P.L. 96-265, and the waiver authority of section 505(a)(3) of that statute and section 1110(b) of the Social Security Act. By conducting the demonstration as a randomized field experiment, SSA took a large step toward substantially expanding what is known about the feasibility and efficacy of rehabilitation and employment services for persons with severe disabilities. The ultimate goal of the demonstration was to return disabled beneficiaries to work, thereby helping them to improve the quality of their lives and lessen their dependence on government income support. The demonstration marked the first time that SSA provided services directly to its client population to help them enter or reenter the workforce. Project NetWork is also the first rigorous, large-scale evaluation of alternative methods of providing such vocational rehabilitation (VR) services.[3]

1.1 Demonstration Design

Project NetWork tested four distinct models for providing employment and rehabilitation services, distinguished by different institutional settings and varying staffing arrangements. Each of the four models was operated for 24 months in two sites during the early- to mid-1990s:

- Model 1, the SSA Case Manager Model (Dallas and Fort Worth), featured the provision of case management services by SSA staff.

- Model 2, the Private Contractor Model (Phoenix/Las Vegas and Minneapolis), also offered case management services, but delivered by private rehabilitation organizations under contract to SSA.

- Model 3, the VR Outstationing Model (New Hampshire and Richmond), featured the provision of case management services by State Vocational Rehabilitation Agencies, with case managers "out-stationed" in local SSA offices.

- Model 4, the SSA Referral Manager Model (Tampa/Carrollwood and Spokane/Coeur d'Alene), offered a less intensive service, referral management, provided by SSA staff. Referral managers were to locate case management and other services for clients by accessing existing service providers in the community.

3 Rupp, Bell, McManus (1994).

Following a three-month pilot period during which demonstration procedures were tested and refined, all sites operated the demonstration for a total of 24 months. The first 15 months were dedicated to both solicitation of participants and providing services to eligible volunteers. The final nine months were dedicated solely to continued service for all active participants.

Participation in Project NetWork was voluntary, and members of the target population were eligible to participate regardless of age, type or severity of disability, or other factors used in traditional vocational rehabilitation programs to screen out individuals judged not to be promising candidates for rehabilitation. In addition to providing case management/referral management services, the demonstration waived specific SSDI and SSI program rules considered to act as work disincentives. These work incentive waivers were intended to encourage participation in Project NetWork and the return to work activities it promoted, by assuring that participants would not be made worse off by these actions. For SSDI beneficiaries, a special waiver exempted earnings for a 12-month period when computing trial work period months and prevented benefit suspension for those who already had exhausted their trial work periods.[4] For SSI recipients, the waivers created a 12-month period during which earnings above $500 per month would not trigger the medical review of disability or blindness that could normally occur at that point.[5]

Individuals who responded to demonstration outreach met with local demonstration staff and were provided a detailed explanation of the demonstration and the opportunity to volunteer. Those wishing to volunteer were then randomly assigned to the treatment group, which received the case/referral management services and the waivers or to the control group, which received only the waivers. Following random assignment, those assigned to the treatment group met individually with a case or referral manager who arranged for necessary assessments, developed an individual employment plan, and identified and arranged for rehabilitation and employment services needed to achieve the plan.

1.2 Evaluation of Project NetWork

In 1992, Abt Associates was awarded a contract to evaluate the effects of Project NetWork. The evaluation featured a randomized experimental design to estimate the net impacts of the

4 The Trial Work Period (TWP), one of the standard work incentive provisions in the SSDI program, was enacted as part of the Social Security Amendments of 1960 (P.L. 86-778). Each month in which earnings from work exceed $200 or self-employment exceeds 40 hours is counted as a TWP month. The TWP provision allows SSDI beneficiaries to have a total of nine such months during a rolling period covering the most recent five years. During the TWP benefits are unaffected by earnings. At the end of the TWP, a determination is made concerning the beneficiary's ability to sustain earnings at the substantial gainful activity (SGA) level. If earnings are lower than SGA levels, regular SSDI eligibility is continued. If earnings have consistently exceeded SGA during the Trial Work Period, cash benefits are then continued during a three-month grace period, and the beneficiary simultaneously enters the 36-month Extended Period of Eligibility during which cash benefits are received in any month in which earnings fall below SGA and withheld in any months in which earnings exceed SGA.

5 As reported to us by SSA officials, however, these continuing disability reviews were not being conducted anyway at the time of the demonstration. This implies that the existence of the waivers did not change the situation SSI recipients faced from what would normally occur.

demonstration on participant employment, earnings, receipt of transfer benefits, social and psychological well-being, and other outcomes. The evaluation included the collection of various types of data: SSA administrative data from the Master Beneficiary Records (MBR), Supplemental Security Records (SSR), and Master Earnings Files (MEF); automated data from the demonstration sites; information on the demonstration from visits to sites; and data from in-person baseline and followup interviews with treatment and control group members and a baseline interview with eligible nonparticipants. A process analysis, assessing the implementation and operations of the demonstration, was completed in 1996.[6] A participation analysis completed in 1998 explored the decision to participate in Project NetWork and participation rates by key subgroups of the eligible population.[7] In addition, the evaluation explores the effects of the work incentive waivers offered to treatment and control group members to encourage their participation in the demonstration and their attempts to return to work. A companion evaluation report assesses the effects of these waivers on the earnings of demonstration participants.

1.3 The Current Report

This report presents the results of the evaluation of the net impacts, costs, and benefits of Project NetWork. The impacts of Project NetWork on employment, earnings, receipt of disability benefits, receipt of rehabilitation and employment services, health and well-being, and other outcomes are examined. The impact analysis was designed to answer four key research questions regarding the efficacy of the demonstration. These include:

- Does case/referral management increase the employment and earnings of project participants?

- Is the receipt of disability benefits reduced? By how much?

- Are there other individual or social benefits from the demonstration intervention, such as increased participant well-being or additional tax collections?

- Will benefits be sustained over time and eventually exceed demonstration costs, producing net benefits to society and/or participants?

It is important to bear in mind that the analysis presented here concerns the incremental impacts of the case/referral management services, above and beyond impacts generated by the waivers (for SSDI beneficiaries) and whatever services the same individuals would have received absent the demonstration. As noted previously, all demonstration volunteers qualified for the special work incentive waivers that allow them to increase their earnings without jeopardizing their disability

6 Wood *et al.* (1996).

7 Burstein *et al.* (1999).

benefits. Likewise, all volunteers, even those assigned to the control group, remained eligible for any employment assistance already available in their communities. The difference between the treatment group and control group, then, is the addition of SSA-sponsored case/referral management services for the randomly selected treatment group. The net impact analysis therefore is concerned with this change alone. The methodology used to estimate net impacts is described in detail in Appendix B. A separate evaluation report (Burstein *et al,*. 1999) explores the effects of the waiver provisions.

1.3.1 Hypothesized Effects of the Project NetWork Demonstration

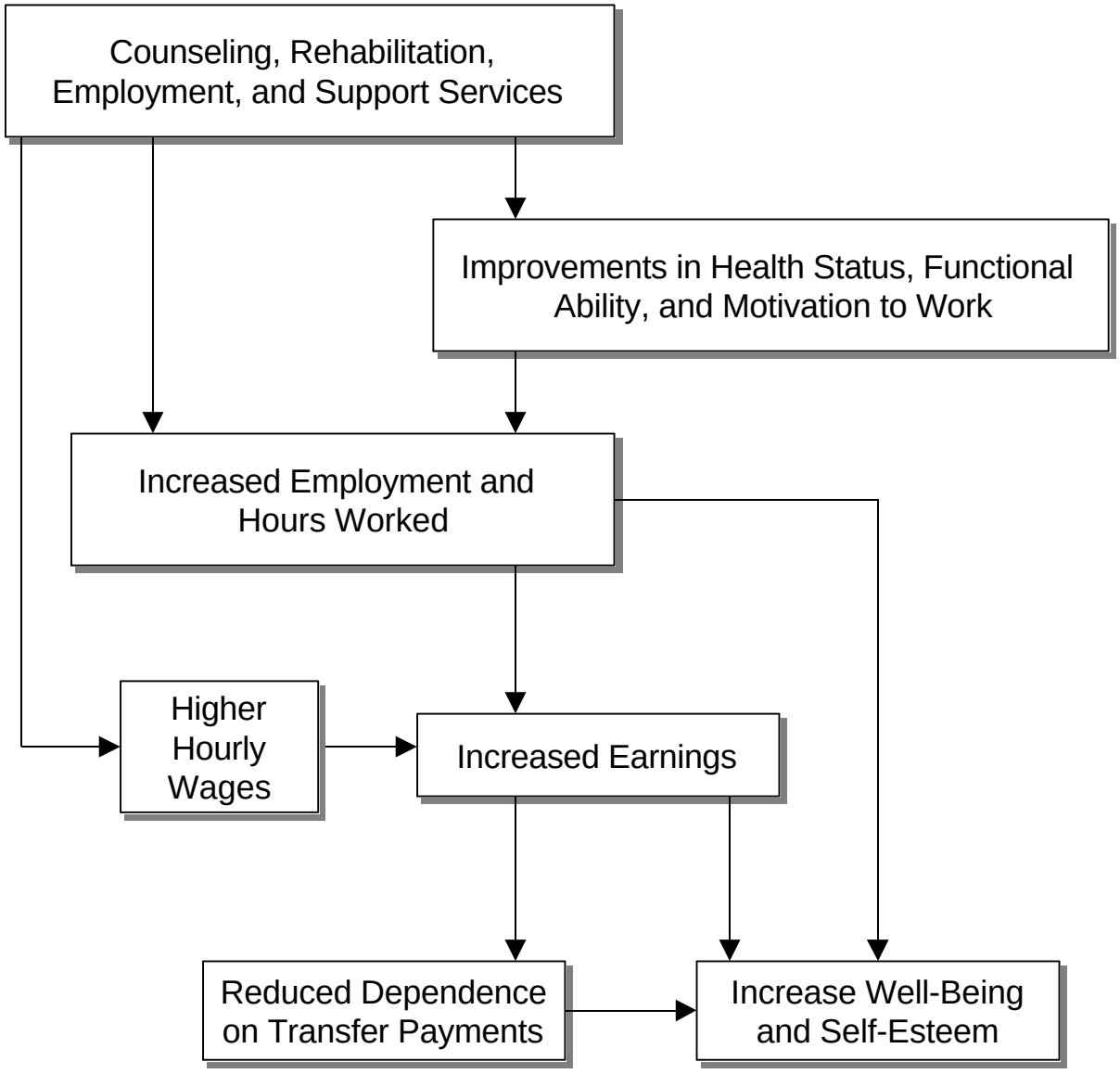
In Exhibit 1.1 the hypothesized effects of Project NetWork's case and referral management services are illustrated. Each of the demonstration models provided counseling, followup, and support services typically delivered by demonstration staff, and rehabilitation and employment services generally provided by outside vendors. The counseling and ongoing support may have given Project NetWork clients information that improved their employment planning, increased their access to rehabilitation and employment services, and/or enhanced their motivation to work. The rehabilitation and employment services could be expected to address specific barriers to employment.

These demonstration services were intended to increase participants' employment prospects, subsequent job retention, and earnings. Some of this may be a *direct* effect of the employment-related services, such as job placement, job search activity, and/or vocational skills training. Effects could also be expected to occur *indirectly* through improvements in health/functional status or outlook about working.

The net effect of these changes should be to increase participant employment and earnings. Earnings effects could occur through an increase in the proportion of recipients working, in hours worked, in hourly wages, or a combination of these factors.

An important consequence of earnings gains would be to reduce participants' transfer income, that is, income derived from SSI, SSDI, and other sources of assistance. While waiver provisions prevent reductions in SSDI and SSI benefits for the first 12 months after a recipient accepts employment, reductions are anticipated after that point, if work activity continues. Health and psychological outlook could also change due to the demonstration, presumably in a positive direction.

Exhibit 1.1
Hypothesized Effects of Case/Referral Management



1.3.2 Data Sources

The two key sources of data used in the analysis of demonstration impacts are SSA administrative data from the MBR, SSR, and MEF files and in-person interviews with treatment and control group members conducted at baseline and follow-up. Appendix A provides additional details on these data sources.

SSA administrative records are an important source of data for the evaluation. SSA administrative records provide an excellent source of information on two key outcomes of interest: the receipt of SSDI and SSI benefits. These data, taken from the MBR810/811 file for DI beneficiaries, and the SSR831 file for SSI recipients, are available on a monthly basis, providing an essentially complete benefit history during the predemonstration period, as well as for the postdemonstration period up until the time of data extraction.[8] For example, the SSR831 was used to collect individual-level data on monthly receipt of SSI benefits, covering the period of January 1990 through December 1996. Each month, information is available on the earned and unearned income used in the calculation of benefits, as well as the federal and state supplementation amounts. For SSDI beneficiaries, monthly receipt of benefits was collected from the MBR810/811 for the period of January 1990 through December 1997.

The SSA administrative records provide information on another important outcome variable – SSA covered earnings. These data were taken from the MEF, which contains annual earnings information on SSA-covered earnings. To ensure the confidentiality of these data, all analysis was conducted by SSA/ORES staff. In addition to data on benefit receipt and earnings, SSA administrative records were used to collect information on the primary impairment and basic demographic characteristics.

In-person interviews are another important source of data for the evaluation. We conducted baseline interviews with a sample of treatment, control, and nonparticipant cases in all demonstration sites over the period of March 1993 through December 1994. A total of 3,439 baseline interviews were completed, including 2,555 with treatment and control group members, and 884 with nonparticipants. The baseline survey used a stratified design to balance the sample across the eight demonstration sites and to oversample SSI applicants and recipients age 18-30. Response rates were 87 percent for participants, 53 percent for existing beneficiaries and recipients sampled as nonparticipants, and 49 percent for new SSI applicants sampled as nonparticiants.

Follow-up interviews were attempted with all treatment and control group cases who completed a baseline survey and who were randomly assigned on or after June 1, 1993, a total of 1,836 cases. Restricting the sample to those who were randomly assigned later in the demonstration was done to ensure that the maximum length of followup for any respondent would be 36 months.

8 See Fu Associates, Ltd (1998a and 1998b) for more details on the creation of the SSI and SSDI benefits analysis files.

Altogether, 1,521 follow-up interviews were completed, for a response rate of 83 percent. Across the two waves of interviews, the combined response rate was therefore 72 percent.

The baseline survey contained questions about education and training, health, functional and activity limitations, employment history, and knowledge of SSA work incentives for disability beneficiaries. The survey also contained a wide array of questions about emotional stability, drug/alcohol use, and cognitive functioning. The followup survey questionnaire contained questions on health and functional limitations, education and receipt of training and rehabilitation services, transportation and child care, employment, personal attitudes and outlook, and income and benefits. The followup survey also provides respondent assessment of Project NetWork and measures of the extent to which participants understand the rules determining SSI and SSDI benefit levels and eligibility, and what effect the demonstration waivers have on these rules.

1.4 SSA's Disability Programs

SSDI and SSI are the two largest federal programs serving people with disabilities. Both programs are administered by SSA and benefits determinations are made using a common definition of disability for both programs. Specifically, disability is defined as the "inability to engage in substantial gainful activity by reason of a physical or mental impairment... [that is] medically determinable and expected to last for not less than 12 months, or to result in death."[9] Other eligibility criteria differ. SSDI was established in 1956 under Title II of the Social Security Act as a social insurance program, with eligibility for disabled workers conditioned on sufficient employment in SSA covered-employment. The SSDI program is funded through Federal Insurance Contributions (FICA) taxes paid into a trust fund by employers and workers. SSI is a means-tested program authorized under Title XVI of the Social Security Act for the aged, blind, or disabled. Unlike SSDI beneficiaries, SSI recipients need not have a work history to be eligible for benefits, but must have low income and limited assets. SSI is funded through general revenues. States have the option of supplementing the basic federal SSI payment. In 1998, all but eight states supplemented federal SSI payments.

In 1996, a total of $44.1 billion was paid to 4.4 million SSDI beneficiaries; in that same year 5.2 million disabled SSI beneficiaries received $21.8 billion in federal benefits.[10] The average monthly SSDI benefit in 1996 was $703.90, and the average federal payment for SSI was $368.21.

9 U.S. House of Representatives, Committee on Ways and Means (1998).

10 Social Security Bulletin Annual Statistical Supplement 1997. Figures for annual payments are taken from Tables 7.A.4 (SSI) and 4.A.6 (SSDI). Total beneficiaries correspond to December 1996 figures from Tables 5.J.8 and 5.D.1(SSDI) and Tables 2.A.1 and 7.A.1 (SSI). Average monthly payments correspond to December 1996 figures from Tables 5.E.2 (SSDI) and 7.A.1 (SSI).

In addition to its potential helpfulness to beneficiaries, interest in rehabilitation and in effective return-to-work strategies is motivated by the potential for substantial benefits savings that could arise from shortening the long and increasing expected duration on the rolls.[11]

1.5 Employment for People with Disabilities

Adults with severe disabilities are employed at much lower rates than adults who do not have disabilities. Using data from the Census Bureau's Survey of Income and Program Participation (SIPP), McNeil (1997) found that in 1991-1992, 24.1 million Americans had a severe disability (as defined by the SIPP). Employment among those with a severe disability was 23 percent, compared to 76 percent among those with a nonsevere disability and 81 percent among those with no disability.[12]

Several factors have been cited as influencing the employment decisions of persons with disabilities. In particular, West and Daniels cite disability-related expenses necessary for some people to work, for such things as assistive technology or personal assistance services, as a potential deterrent to seeking employment. Also, they note that the recurring or cyclical nature of some disabilities may necessitate flexible work schedules in order for employment to be a feasible option.[13] Others, including the Presidential Task Force on Employment of Adults with Disabilities, have suggested that discrimination against persons with disabilities persists and affects employment opportunities.

Oi and Andrews (1992) described several factors that make employment choices more complex for persons with disabilities than for their counterparts without disabilities. Disabilities can affect an individual's labor supply and demand for training services in a variety of ways:

- Persons with disabilities may have less time available for work or leisure than persons without disabilities, after allowing for necessary maintenance activities (including physician visits, periods of acute illness, and the extra time and effort required for daily activities).

- An individual's productivity may be affected by her or his impairment. As a result, the wage that person can command is typically lower than for other workers.

11 Rupp *et al.* (1996).

12 See also Hale, et al. (1998) and Burkhauser and Daly (1996). Those with severe disabilities are defined by the SIPP as those who report having a physical, mental, or other health condition which limits the kind or amount of work they can do and who have difficulty with one or more Activity of Daily Living (ADL) (e.g., walking, eating, bathing) or Instrumental Activities of Daily Living (IADLs), such as shopping or working.

13 Daniels and West (1998).

- Where statutes or custom prevent employer adjustment of wages to reflect perceived lower productivity or higher fringe benefit costs (especially health insurance costs), employers may be reluctant to hire persons with disabilities.

- The individual's costs of employment, including special costs of transportation, assistive devices, and so forth, may be higher than for persons without disabilities.

- Life expectancy may be reduced because of the impairment, and hours worked per week may be lower, lowering the potential returns to training/job search investments.

As Burkhauser (1998) and others have noted, there is great diversity within the population with disabilities with respect to severity of the disability and the skills they bring to the workforce. While we do not expect that all persons with disabilities will work, there may be a substantial minority for whom employment is a viable and desired option.

1.6 SSA's Efforts in Promoting Return-to-Work Among Disability Beneficiaries

Rehabilitation and employment services have been federally funded (currently through the Rehabilitation Services Administration of the U.S. Department of Education) and administered by state vocational rehabilitation (VR) agencies since 1920. State VR agencies receive referrals of persons with disabilities from many sources, including hospitals, mental health agencies, and schools. In 1965, SSA initiated its Beneficiary Rehabilitation Program (BRP) to fund rehabilitation and employment services for disability beneficiaries through these VR agencies. Initially, funding for these services was granted annually to state VR agencies to finance the full cost of services to SSDI and SSI beneficiaries, regardless of the outcome of the services.[14] In response to concerns about the cost-effectiveness of the VR services, Congress changed the method of funding in 1981 (P.L. 97-35). Since then, a retrospective payment system has been in place in which service costs are reimbursed only for beneficiaries who, as a result of the VR services received, engage in substantial gainful activity for at least nine consecutive months.[15] The current reimbursement system, representing SSA's first step in moving toward an outcome-based system of financing return-to-work services for its beneficiaries, therefore provides strong incentives for state agencies to serve only those whom it believes can be successfully rehabilitated.

14 Rupp, Bell, McManus (1994).

15 U.S. House of Representatives, Committee on Ways and Means (1998) and Berkowitz and Dean (1996).

The Social Security Act requires that applicants for disability benefits be referred to state VR agencies to receive rehabilitation services, in cases where such referrals are deemed useful.[16] State Disability Determination Services (DDSs), the agencies that make disability determinations for both the SSDI and SSI programs, are responsible for making these referrals. In 1996, the U.S. General Accounting Office (GAO) reported that, on average, DDSs refer only about 8 percent of SSDI and SSI applicants who are awarded benefits. In a 1987 study, the GAO found that less than 10 percent of beneficiaries referred by DDSs were accepted by VR agencies as clients. The remainder were not considered feasible prospects by the state agencies, did not respond to the agency contact, were uninterested in VR services, or were already known to the agencies. Moreover, the GAO reported that only about 1 of every 1,000 beneficiaries per year is rehabilitated through the VR referral process.[17]

In the expectation that many more disability beneficiaries can participate in rehabilitation programs and successfully return to work if given the right incentives and support services, the Social Security Administration initiated a series of demonstration projects during the 1980s. Most of these demonstrations focused on increasing individuals' access to rehabilitation and employment services, and on increasing awareness of existing work incentive provisions in SSDI and SSI program regulations. Others tested the efficacy of private sector and nonprofit organizations in delivering services, or innovations in service delivery such as case management, expanded on-the-job training, business internships, and post-employment training.

With the exception of the Transitional Employment Training Demonstration (TETD) undertaken in the mid-1980s, however, none of these demonstrations were rigorously evaluated. Thus, the Project NetWork demonstration, because of its scale and rigorous evaluation design, offers a wealth of previously unavailable information about the feasibility and efficacy of rehabilitation and employment services for persons with severe disabilities.

The Transitional Employment Training Demonstration (TETD) was undertaken by SSA from 1985 to 1987 to increase the economic and social self-sufficiency of SSI recipients with mental retardation.[18] The demonstration served SSI recipients aged 18-40 with a diagnosis of mental retardation in their SSI files. The case folders of 30,000 SSI recipients were screened, and 13,800 eligible people were identified. Of those, 745 (5.3 percent) enrolled. Five core services were offered, including intensive outreach; waivers to SSI regulations ensuring that recipients

16 Daniels and West (1998). See also GAO (1996), in which SSA's national guidelines for these referrals are described. In general, SSA counsels DDSs to refer all SSDI and SSI applicants for VR services, except those with terminal illnesses, severe or rapidly progressive impairments not responding to treatment, or other characteristics that make rehabilitation and sustained work unlikely. In addition to these national guidelines, the GAO reports that some DDSs have worked with State VR agencies to develop additional criteria for screening out certain types of beneficiaries for referrals.

17 This refers to SSA's standard for rehabilitation, nine consecutive months of employment at the substantial gainful activity (SGA) level (currently $500 per month). State VR agencies use a less stringent definition, in which placement in suitable employment, whether paid or unpaid, for a period of 60 days is considered successful rehabilitation.

18 Thornton (1998).

choosing to enroll could maintain their eligibility for SSI benefits; placement in potentially permanent competitive employment; on-the job training; and post-placement support and followup as necessary for job retention. The evaluation of the TETD found that over the six-year followup period, the impact of the TETD services on average employment and earnings levels was statistically significant, proportionally large, and relatively persistent. Average earnings for treatment group members were found to be 73 percent higher than for control group members over the same period. Despite the proportionately large impact on earnings, the absolute change was found to be relatively small; average cumulative earnings rose $4,282 for the six years ($714 per year). The impact on SSI payments was statistically significant but relatively small; over the six years, payments fell by an average of $870, or 5 percent. A cost-benefit analysis showed that the costs of the demonstration exceeded the reductions in SSI payments that SSA realized.

SSA's more recent efforts to increase the number of beneficiaries with disabilities who work represent a departure from the case management approaches tested under Project NetWork, focusing instead on providing greater incentives for public and private sector providers of employment and rehabilitation services and enhanced beneficiary choice. In 1997, SSA initiated the Alternate Participant (AP) program, the first substantial effort aimed at tapping the resources of private rehabilitation service providers. The AP was implemented under existing legislative authority as an enhancement to the current VR referral program to expand the opportunities for beneficiaries to receive VR services. Under the AP program, the law requires that SSA continue to make the first referral for rehabilitation services to the State VR agency. If state VR agencies fail to notify SSA within a specified period of time that a referral has been accepted for services, referrals can be made to approved alternate providers. Alternate providers were selected through a competitive process beginning in March 1997, and to date nearly 400 providers have signed contracts with SSA to provide employment and rehabilitation services.

Another major change to the current SSA VR program is the Ticket to Independence program that was proposed by the Administration in 1997. Under this proposed program, a beneficiary with a long-term impairment would be issued a ticket providing access to a broad range of employment and rehabilitation services. The tickets could be given to a provider chosen by the beneficiary in exchange for rehabilitation and employment services. The certified private and public providers who are offered and accept a ticket would be compensated only after the beneficiary is placed in a job and achieves independence from SSA's disability benefit rolls. The providers would be paid a portion of the benefits savings realized by SSA as a result of the beneficiary's work activity.

In June 1998 the House passed H.R. 3433, the "Ticket to Work and Self-Sufficiency Act of 1998" which included the Ticket to Independence program and many of the elements of SSA's proposal. This program has also been included in a Senate bill, S.331, the "Work Incentives Improvement Act of 1999", that is being considered by the Senate.

1.7 Organization of the Report

The remainder of the report is organized as follows. Chapter 2 summarizes the key findings from the process analysis, and Chapter 3 describes the baseline characteristics of the research sample. In Chapters 4 and 5, impacts of the demonstration on earnings, SSI and SSDI benefits, and other outcomes are reported. Chapter 6 shows the results of the analysis of demonstration's net costs and benefits. Appendix A provides details on the sources of data for the analyses, and Appendix B provides technical details on the estimation of demonstration impacts. In Appendix C, we provide additional results from the analysis of impacts on earnings and employment, and in Appendix D we provide additional results from the analysis of impacts on benefit receipt. Appendix E describes the methodology used in the estimation of demonstration costs and benefits.

Chapter 2
Project NetWork Implementation and Operations

In previous reports we presented the results of the Project NetWork process study.[1] The key findings from that analysis are reviewed here as context for the estimates of demonstration impacts presented in subsequent chapters. The process study examined the implementation and operations of Project NetWork. The study used data collected during site visits including interviews with local demonstration staff and reviews of a small number of participants' case folders and data from the Case Management Control System (CMCS), the automated client tracking system maintained in each site to record demographic information and the completion of various demonstration milestones. In addition, the process study presented secondary data from the Bureau of Labor Statistics and the 1990 Census describing local labor markets and demographic characteristics of the communities in which the demonstration operated.

The analysis described the implementation and operation of Project NetWork by focusing on issues of operational feasibility and the differences and similarities among the different service provision models. Specifically, the process study examined the organization, staffing, and management of the demonstration in each site, as well as the number and personal characteristics of demonstration participants assigned to the treatment group. It focused particularly on the nature of case/referral management services and the progress of individual clients through the case/referral management process. Four major steps were analyzed: recruitment and intake; client assessment; development of the Individual Employment Plan (IEP); and provision of rehabilitation services.

2.1 Organizational and Operational Differences Among Models

The four distinct models, with their varying organizational arrangements and staffing patterns, resulted in different operating environments for the demonstration. The models differed in several ways. First, the service offered in Models 1, 2, and 3—case management—was more intensive and comprehensive than the referral management provided in Model 4. Case managers in Models 1, 2, and 3 were required to decide whether or not to extend rehabilitation services to participants based on medical, psychological, and vocational assessments; establish a vocational goal and services plan; monitor participants' progress towards reaching that goal; and modify the services offered as needed. Case managers counseled their clients as they coordinated their rehabilitation process. Referral management (the service offered in Model 4) consisted of referring participants to rehabilitation service providers who performed the case management function. Another distinguishing feature of the SSA Referral Manager model was that referral managers were

1 Wood et al. (1996) and Leiter et al. (1997).

encouraged to refer participants whenever possible to agencies whose services could be provided without cost to the demonstration.

Staff in the SSA Case Manager model and the SSA Referral Manager model (Models 1 and 4) were former SSA claims and service representatives and therefore had less experience in vocational rehabilitation and case management than staff in the Private Contractor model and the VR Outstationing model (Models 2 and 3). As an additional resource for the Model 1 SSA case managers, each Model 1 office was supported by a field consultant who was an experienced vocational rehabilitation counselor. In addition, the SSA case managers received the longest training of all models, with nine weeks of formal classroom training supplemented by in-service training offered by the consultants. Since the SSA referral managers were not intended to provide case management to their clients, these sites did not include field consultants in their staffing, nor did the referral managers receive classroom training in vocational rehabilitation procedures.

The private contractor case managers (Model 2) were experienced case managers and many had prior experience with vocational rehabilitation, as originally planned. The original design of the VR Outstationing Model envisioned that these case managers would be experienced vocational rehabilitation counselors from within the state VR agencies that ran the demonstration. Moreover, these counselors were stationed in local SSA offices, away from other VR operations. This model did involve outstationing as planned, but the majority of its case managers were hired from outside the VR system, at variance with the original design. Some of the case managers who were hired from outside VR previously worked in private vocational rehabilitation, but others had no prior experience in the field, and some had no prior case management experience.

The SSA case managers had the smallest caseloads, an average of 73 clients per case manager over the course of the demonstration. As expected, given the nature of referral management, the SSA Referral Manager model (Model 4) had the highest caseloads, with an average of 114 clients per referral manager. The staffing was quite stable in the SSA Case Manager and SSA Referral Manager models (Models 1 and 4), with little turnover among case/referral managers. All of the staff in these sites were SSA employees and were guaranteed a return to their previous jobs at the conclusion of the demonstration. In the Private Contractor and VR Outstationing models (Models 2 and 3), however, positions were temporary, with no assurance of employment after Project NetWork concluded. As a result of this relative insecurity, a great deal more staff turnover occurred in these models. Overall, turnover was not believed to have had a detrimental effect on demonstration operations since the replacement staff were highly qualified; however, toward the end of demonstration operations, the quality of replacement staff declined.

2.2 Key Findings from the Process Analysis

The process study established a picture of intake and service delivery in the demonstration that is essential to keep in mind when considering the net impacts of the demonstration. That study produced the following key findings.

First, we now know that some people who receive SSDI and SSI disability benefits are interested in the possibility of becoming employed and that all four treatment models can succeed in recruiting these potential participants. A total of 8,248 people across the eight sites volunteered for the program (approximately 4.5 percent of those who were solicited[2].) Among the recruitment methods that were used, the quarterly mailings contributed the greatest number of people (60 percent of all volunteers). However, this method could have been improved by sending out letters at a more even pace (e.g., on a monthly basis). The quarterly mailings created response backlogs that made it difficult for case/referral managers to respond to interested individuals in a timely manner.

Several of the other recruitment methods were observed to have had limited success. For example, few participants were recruited from follow-up mailings to targeted beneficiaries. Also, the attempts to solicit new SSI applicants in the SSA claims office produced mixed results. Other options, such as outreach mailings following benefit award should be considered for this important group.

Once clients were assigned to the treatment group, the managers obtained diagnostic assessments of their medical and psychological conditions. Substantial delays were encountered in obtaining diagnostic assessments, which pushed back the development of the IEP and provision of rehabilitation services. Case managers reported that some clients lost interest in Project NetWork during this waiting period.

It could also take a long time to obtain vocational assessments. Vendors sometimes had long waiting lists, which could cause a manager to wait up to 90 days before receiving an assessment report on a client. Some of the case managers avoided these delays by performing vocational assessments themselves. Case managers who had previous training or experience were more likely to do this. Many reported that it helped them to get to know their clients' needs better and establish a rapport with them. The referral managers in the SSA Referral Manager model did not have the training or background needed to perform vocational evaluations, and were dependent upon other professionals to do them and make recommendations.

Overall, 60 percent of all treatment group members reached the next step in the return-to-work process, the development of an Individual Employment Plan (in the case management models), or an Individual Referral Plan (in the referral management model). On average, the length of time to complete an IEP was longer in the case management sites than in the referral management model. The average number of days from random assignment to IEP completion ranged from 76 to 138 in the case management models, while the average number of days in the referral management model was 20 in one site and 60 in the other. This difference is consistent with the demonstration design, which calls for a more intensive IEP development process than the IRP

2 See Burstein et al. (1998) for a full description of the participation decision, and rates of participation across key subgroups.

process. The managers in all of the sites reported that they would have liked more training on developing these plans.

After the case/referral manager and the client completed the IEP/IRP, the client was eligible to receive rehabilitation and employment services. These services could be purchased from outside providers, acquired from other outside providers at no cost to the program, or provided directly by the managers. Overall, 45 percent of all treatment clients received purchased rehabilitation services. Job development and placement services were purchased most frequently; 30 percent of all treatment clients received these services.

The Project NetWork follow-up survey provides information on the treatment group's receipt of case/referral management services as well as their perceptions regarding the helpfulness of Project NetWork. This information is summarized in Exhibit 2.1. As shown, 89 percent of the treatment group members who responded to the survey said that they had met with a case/referral manager at least one time. Approximately half said they received services from Project NetWork.

When asked about the number of times per week they typically met with a case manager, most (60 percent) said less than once per week. About a fifth of the treatment group members reported meeting with their case/referral manager only once ever. Nearly half of the treatment group members who reported at least one in-person meeting with a case/referral manager said that these meetings lasted an average of 30-60 minutes, while 30 percent reported shorter meetings of between 15 and 30 minutes.

Finally, we asked all treatment group members about their opinions of the helpfulness of Project NetWork and found that their perceptions were positive. For example, 72 percent strongly agreed or agreed that they got help from their case/referral manager when needed. One-third strongly agreed or agreed that Project NetWork helped them find a job, and a full 77 percent said that if they had the choice to make over again, they would participate in Project NetWork.

Exhibit 2.1
Satisfaction with Project NetWork Services

	Percentage of Treatment Group Survey Respondents (N=786)
Ever met with case manager in person	89%
Received Project NetWork services	49
Number of times per week met with case/referral manager	
Only one time ever	18
Less than once per week	60
1-2 times per week	18
3-4 times per week	1
5 or more times per week	.6
Average length of in-person meetings with case/referral manager, for those who reported at least one meeting	
<15 minutes	9
15-30 minutes	30
30-60 minutes	49
>60 minutes	11
Ever had contact by phone with case/referral manager	84
Percentage of Survey Respondents who Strongly agree or agree with the following statements...	
I got help from my case/referral manager when I needed it	72
Project NetWork helped me get a job	33
If I had it to do over again, I would participate in Project NetWork	77

Source: Project NetWork Follow-up Survey

The main finding in the Project NetWork process analysis is that all of the demonstration models were able to recruit large numbers of participants and to provide rehabilitation and employment services to their clients on a substantial scale. Thus, despite the differences in the ways that the sites were organized, staffed, and managed, all but one of the sites was able to meet its recruitment goals. And, once brought into the project, most clients completed assessment and reemployment planning and received some employment-related services in all models.

In addition to this broad lesson on **operational** feasibility, the Project NetWork process study provided a second, perhaps more surprising result: *broad-based return-to-work efforts can be implemented on a large scale in a variety of institutional arrangements*. While the details differ, the three types of organizations asked to implement case management in the demonstration succeeded on approximately the same scale in recruiting and working with very comparable populations. Experienced vocational rehabilitation organizations, both public (the VR Outstationing model) and private (the Private Contractor model), got roughly the same results as newly-created in-house entities of SSA (the SSA Case Manager Model), at least in terms of the number and type of clients recruited and served and the percentage of clients brought to each step of the return-to-work process.

Sharper differences emerged when the intensity of services was varied rather than the organizational arrangement. The SSA Referral Manager model obtained similar outreach and intake goals as the three case management models, but produced qualitatively and quantitatively different results at the assessment and planning stages (less assessment and more sketchy planning) while relying substantially less on purchased employment services. These patterns do not so much point to deviations from a desired norm as affirm yet again the operational success of the demonstration generally, since all of the differences noted are consistent with the distinctive design of the referral management approach. Thus, while tried in only a single setting (in-house at SSA), the referral manager approach also appears to have met its operational goals and expectations.

2.3 Services Received by Treatment and Control Group Members

Volunteers who were randomly assigned to the control group were offered the demonstration waivers but not the case/referral management services available to the treatment group. Nevertheless, control group members were free to seek any other rehabilitation and employment services available in the community outside of Project NetWork. The analysis of net impacts presented in this report therefore measures the effects of Project NetWork case/referral management services over and above what the control group received in the absence of the demonstration.

The process study examined the nature of the case/referral management services provided by Project NetWork to the treatment group. Using data from the automated system that tracked

purchased services, we described the types of services that case and referral managers purchased for their clients. These data serve an important purpose in assessing the costs of demonstration services (see Chapter 6), but they do not include services obtained for Project NetWork participants at no cost to the demonstration, nor do they provide an accounting of services received outside of Project NetWork, by either the treatment or control group. To measure the receipt of services from all sources, by both the treatment and control group, we collected information in the follow-up survey about the receipt of several types of rehabilitation and employment services. Treatment-control comparisons of these measures tell us what types of *incremental* services produced the impacts reported in Chapters 4 and 5.

Respondents to the follow-up survey were asked whether or not they had received a variety of services at any time since random assignment. The categories of services included:

- Job search assistance/career guidance;
- Business skills training;
- Job-related training;
- Other rehabilitation/training;
- Life skills training;
- Occupational therapy;
- College classes;
- Assessment of work potential;
- Physical therapy; and,
- Psychological counseling.

The survey also collected information about the services received, including their duration, whether or not they were provided by Project NetWork or state VR, the number of hours per week the service was received, and whether or not the service was helpful in finding a job. Data were collected for up to seven episodes of service receipt for each type of service.

Exhibit 2.2 presents treatment-control comparisons of the receipt of each type of service. The information presented includes the mean for the control group and the treatment group mean, regression-adjusted to account for differences in the measured baseline characteristics of the treatment and control groups.

Exhibit 2.2
Receipt of Education, Training, and Rehabilitation Services Full Sample
(percent of each group receiving each service)

	Control Group Mean	Treatment Group Mean
Since random assignment, participated in...		
Job search assistance	14%	21%***
Business skills training	6	11***
Job-related training	10	12
Other rehabilitation/training	2	1
Life skills training	6	6
Occupational therapy	4	4
College classes	10	8
Assessment of work potential	17	27***
Physical therapy	23	23
Psychological counseling	38	41
Any service	69	75**

* Treatment/control difference statistically significant at the 10 percent level
** Treatment/control difference statistically significant at the 5 percent level
*** Treatment/control difference statistically significant at the 1 percent level

Treatment Group means presented are regression-adjusted to account for differences in the measured baseline characteristics of the treatment and control groups.

Sample sizes:
786 in the treatment group, 735 in the control group, 1,521 in total

Source: Project NetWork Follow-up Survey

As the exhibit shows, the control group members received a substantial amount of services. A full 69 percent of the control group reported receiving any of these services, compared with 75 percent of the treatment group (difference is significant at the 5 percent level). Regarding specific services, 38 percent of the control group reported receiving some psychological counseling over the follow-up period, and 23 percent reported receiving physical therapy. Treatment group members received, on average, significantly more job search assistance, business skills training (training in a trade or business school), and assessment of work potential than did their counterparts in the control group. The incremental services received by the treatment group are therefore concentrated in these three types of services. Any net impacts observed in terms of earnings, receipt of SSDI or SSI benefits, and other outcomes can be attributed to these service differentials, plus the counseling and support provided by the case/referral managers. Similar information on the receipt of services, by type of primary impairment, are shown in Appendix B.

It is important to note that, even where statistically significant, these treatment-control service differentials are not large. For example, the proportion of the treatment group that received job

search assistance is only 7 percentage points higher than the proportion of the control group that received this service. Moreover, the proportion of treatment group members who reported receiving any of these services is only 6 percentage points higher than the control group. Although services received by treatment group members may have been more targeted on employment, we would not expect differentials of this size to lead to large impacts on the treatment group as a whole.

Chapter 3
Characteristics of the Demonstration Sites and Research Sample

This chapter describes selected characteristics of the eight Project NetWork demonstration sites and the baseline characteristics of the Project NetWork treatment group. These descriptions provide contextual information useful to interpret the impact estimates presented in later chapters.

3.1 Demonstration Sites

Each of the four treatment models was implemented in two sites. Most of the sites encompassed two or more SSA field offices, typically located near each other geographically. The SSA Case Manager Model (Model 1) sites and SSA Referral Manager Model (Model 4) sites were selected first. An important requirement for Model 1 and Model 4 sites was the interest and cooperation of the Regional Office. SSA Central Office executive staff approached the Regional offices, and once their interest was confirmed, the staff screened possible service areas to ensure they were large enough to supply the number of eligible persons needed to achieve sufficient sample sizes. SSA executive staff also tried to select sites that would provide geographic diversity and that had strong field office management. One of the SSA Case Manager model sites operated in two field offices in Dallas, Texas (Oak Cliff and Dallas North); the other operated in one field office in Fort Worth, Texas. Both of the SSA Referral Manager model sites operated in two SSA field offices each: in Spokane, Washington, and Coeur d'Alene, Idaho; and in Tampa and Carrollwood, Florida.

The SSA Case Manager and Referral Manager Models were staffed by Social Security Administration staff who had previously worked as claims and service representatives. In each site, separate office space within the field offices was designated for the operations of Project NetWork.

The Private Contractor Model (Model 2) and VR Outstationing Model (Model 3) sites were selected through a competitive bidding process. Responding to Requests for Proposals, private contractors and State VR agencies submitted proposals to SSA to operate the NetWork demonstration. The proposals were evaluated by SSA, with consideration given to the experience of the organizations and the merits of the proposed approach. At the same time, consideration was also given to the size of the SSA service areas to ensure a sufficient number of volunteers.

Case management staff in the Private Contractor Model (Model 2) were employees of private organizations. This was the only demonstration model in which day-to-day Project NetWork operations took place in a location other than an SSA field office. The Southwest Business, Industry and Rehabilitation Association (SWBIRA) operated Model 2 in two locations in Arizona (Phoenix and Scottsdale), and in Las Vegas, Nevada. SWBIRA is a private, non-profit corporation formed to link the labor force needs of business and industry with qualified workers with disabilities. Space was set aside within existing SWBIRA offices for operating Project NetWork.

Karr Rehabilitation Services, Inc., a for-profit rehabilitation organization in Minneapolis, Minnesota, operated the other private contractor demonstration site in one location in downtown Minneapolis. Karr Rehabilitation Services is a company specializing in medical and vocational disability management. One of six founding companies of AmeriSys (a national case management firm), Karr maintains permanent offices in 27 locations throughout the Midwest and Northwest. For the purposes of operating Project NetWork, Karr established a new office location in downtown Minneapolis.

The State Vocational Rehabilitation (VR) Outstationing Model was operated by state vocational rehabilitation (VR) agencies in New Hampshire and Virginia under contract to SSA. Case management staff were reassigned to Project NetWork from within those agencies or hired from outside. Another distinguishing feature of this model was that these case managers were outstationed (i.e., stationed away from VR offices) in SSA field offices. Project NetWork case managers were given office space in the local SSA field offices for the operation of Project NetWork. In New Hampshire, Project NetWork was operated statewide in all six SSA field offices: Concord; Keene; Littleton; Manchester; Nashua; and Portsmouth. In Richmond, Virginia, Project NetWork operated in four SSA field offices in and around Richmond.

Exhibit 3.1 displays information about the demonstration sites, including the agencies responsible for operating Project NetWork in each model, the start date of demonstration operations, the size of the eligible population and the total number of volunteers. As shown in the exhibit, services were provided to Project NetWork participants in 20 distinct locations across the eight sites. The existence of multiple offices in separate geographic locations means that Project NetWork effectively operated in a broad range of labor market and service environments. In the following sections we compare selected characteristics of the demonstration sites, including the size of the Project NetWork eligible population, unemployment rates, demographic characteristics, and employment rates among people with disabilities. A more detailed description of the local demonstration service areas is given in Wood *et al.* (1996).

3.1.1 Size of the Project NetWork Eligible Population

Exhibit 3.1 shows the number of people solicited to participate in Project NetWork in each of the demonstration sites. All SSDI beneficiaries and SSI disability/blind recipients and applicants, who lived in the service areas of the eight sites were solicited to volunteer for participation in Project NetWork. Only those individuals already participating in a return-to-work program were ineligible for Project NetWork. Existing beneficiaries and recipients were informed about the demonstration through mailings from the SSA Central Office. Mailings were planned to occur every three months in each site, for a total of five mailings per site over the 15-month period of intake. Each mailing was designed to encompass a 20-percent random sample of the existing caseload at that time.[1] New SSI applicants were told about the program by SSA claims representatives during the application process. The count of persons solicited in Exhibit 3.1 therefore gives an approximation of the size of the eligible SSI and SSDI caseloads in each demonstration site. The largest number of solicitees were in the Phoenix/Las Vegas, Minneapolis, and Tampa sites, all of which had well over twice as many eligibles as in New Hampshire.

3.1.2 Unemployment Rates in the Demonstration Sites

Exhibit 3.2 shows the unemployment rates in the sites in the month in which demonstration operations began, compared to the overall rate for the U.S. at that time. As the exhibit shows, unemployment rates were lower than the national figures in all sites except New Hampshire and Spokane/Coeur d'Alene at the time the demonstration began. The lowest rates of unemployment were in Minneapolis and Richmond (each with 4.8 percent unemployment), while the highest rates were in Spokane (8.9 percent) and New Hampshire (8.3 percent). We also examined changes in unemployment rates during the period of demonstration operations (data not shown). We found that the patterns present at the beginning of the demonstration continued, with New Hampshire and Spokane/Coeur d'Alene experiencing unemployment rates consistent with or higher than national figures throughout the demonstration period, and other sites with unemployment rates consistently lower than national rates.

3.1.3 Demographic Characteristics of the Demonstration Sites

We also examined data from the 1990 Census to compare demographic characteristics of the populations in the demonstration sites to one another and the nation. In particular, we examined educational attainment, age, race, and mean per capita income, as shown in Exhibit 3.3.[2] All sites have higher percentages of the population with either some college or a college degree than the

1 An analysis of the recruitment process and participation decisions is provided in Burstein *et al.*(1998).

2 Each Project NetWork demonstration site consisted of one or more SSA field offices, each with a service area defined by zip codes. We extracted zip-code level data from the 1990 Census STF-3 file. To create aggregate measures from these data we calculated weighted averages, weighting by the number of people residing in each zip code area.

Exhibit 3.1
Characteristics of the Project NetWork Demonstration Sites

Model	Demonstration Agencies	Service Locations	Start of Full Operations	Size of the Project NetWork Eligible Population	Number of Volunteers (Treatment and Control Group Members)
Model 1 - SSA Case Management Model	Two Dallas SSA Field Offices (FO)	Dallas, TX: Oak Cliff FO Dallas North FO	June 1992	17,384	1,147
	Fort Worth SSA Field Office	Fort Worth, TX	June 1992	13,320	752
Model 2 - Private Contractor Model	SouthWest Business and Industry Rehabilitation Association (SWBIRA)	Phoenix, AZ Las Vegas, NV	January 1993	24,520	1,100
	Karr Services	Minneapolis, MN	January 1993	23,803	1,012
Model 3 - VR Outstationing Model	State of New Hampshire Vocational Rehabilitation Agency	Concord, NH Manchester, NH Littleton, NH Nashua, NH Portsmouth, NH Keene, NH	February 1993	9,457	1,083
	State of Virginia Vocational Rehabilitation Agency	Richmond, VA: Richmond FO Richmond East FO Chesterfield FO Richmond West FO	March 1993	18,652	1,131
Model 4 - SSA Referral Management Model	Tampa and Carrollwood SSA Field Offices	Tampa, FL Carrollwood, FL	January 1993	22,728	1,079
	Spokane and Coeur d'Alene SSA Field Offices	Spokane, WA Coeur d'Alene, ID	January 1993	15,540	944
Total				145,404	8,248

Exhibit 3.2
Unemployment Rates at the Beginning of Demonstration Operations, Project NetWork Sites Compared to the U.S.

Site	Demonstration Start up	Unemployment Rate at Demonstration Start Up	Unemployment Rate, U.S.
Dallas	June 1992	7.7	7.7
Fort Worth	June 1992	7.5	7.7
Minneapolis	January 1993	4.8	8.0
Phoenix	January 1993	6.5	8.0
Las Vegas		6.7	
New Hampshire	February 1993	8.6	8.0
Richmond	March 1993	4.8	7.0
Spokane	January 1993	8.9	8.0
Coeur d'Alene		8.1	
Tampa	January 1993	7.3	8.0

A Rates shown correspond to the month in which demonstration operations began. Seasonally adjusted rates not available for individual cities; rates shown, for both demonstration sites and the US, are therefore not seasonally adjusted. Information shown for Coeur d'Alene is the unemployment rate for the state of Idaho in the month the demonstration began.

Sources:U.S. Department of Labor, Employment and Earnings . Data for Spokane taken from Employment Security Department, State of Washington.

Exhibit 3.3
Demographic Characteristics in the Project Network Demonstration Sites in 1990

Percent of Population

	Dallas	Fort Worth	Phoenix/ Las Vegas	Minneapolis	New Hampshire	Richmond	Tampa	Spokane	U.S.
Educational Attainment									
<HS Graduate	19	21	22	13	18	24	25	18	25
HS graduate	22	25	29	26	32	27	28	30	30
Some College	30	33	33	32	28	26	28	35	27
College Degree	21	16	10	21	15	16	13	12	12
Graduate Work	8	5	5	8	7	7	6	5	6
Age									
15 and under	22	24	22	21	23	22	21	24	22
16-39	46	43	41	43	40	37	40	36	40
40-69	27	27	30	28	29	31	30	31	33
70 and over	5	6	7	8	8	8	9	9	5
Race									
White	71	83	85	90	98	71	82	95	80
Black	19	8	4	6	1	28	15	1	12
American Indian	0	0	2	1	0	0	0	2	1
Asian	3	3	2	3	1	0	1	1	3
Other	7	6	7	0	0	0	2	1	4
Mean per capita income	$18,763	$15,589	$15,136	$18,628	$15,769	$16,331	$14,353	$12,225	$14,420
Population size[a]	1,306,046	994,242	1,307,902	1,023,150	1,008,197	786,691	648,750	544,002	248,709,873

[a] Total number of people living within each site's service area, defined by zip codes.
SOURCE: 1990 Census of Population and Housing (Summary Tape File 3).

nation as a whole. Educational attainment is highest in the Minneapolis and Dallas sites, and lowest in Tampa. The age distribution in the demonstration sites is generally similar to the nation overall; however, in all sites except Dallas, the percentage of persons over the age of 70 is higher than for the nation. Racial composition varies among the demonstration sites, reflecting regional differences in population composition. There is less racial diversity in the New Hampshire, Spokane/Coeur d'Alene, and Minneapolis sites, when compared with both the other Project NetWork sites and with the nation as a whole. Greater racial diversity is found in Dallas, Richmond, and Tampa. For example in Dallas, 71 percent of the population is white (including 13 percent of Hispanic origin) and 19 percent is black. In Tampa, 82 percent of the population is white, 15 percent is black, and 14 percent is of Hispanic origin. In Richmond, a full 28 percent of the population is black compared with 12 percent of the population in the nation.

Per capita income is highest in the Dallas ($18,763) and Minneapolis ($18,628) sites. All sites except Tampa and Spokane, however, have per capita income levels above the national figure. The lowest income level is in the Spokane site ($12,225).

3.1.4 Employment Experiences of People with Disabilities

As discussed in Chapter 1, the Census Bureau's Survey of Income and Program Participation (SIPP) provides information regarding the incidence of disability among the general population and the employment experiences of people reporting a disability. McNeil (1997) provides a summary of these data for the last three months of 1991 and first month of 1992. He found that 24.1 million Americans reported having a severe disability. Those with severe disabilities are defined as those who report having a physical, mental or other health condition which limits the kind or amount of work they can *and* who have difficulty with one or more Activity of Daily Living (ADL) (e.g., walking, eating, bathing) or Instrumental Activity of Daily Living (IADL) such as shopping or working.

McNeil also found that the employment rate for persons with a severe disability was 23 percent, compared with 76 percent among those with a nonsevere disability and 81 percent among persons with no disability. (By 1994, the employment rate had increased to 23.3 percent among the 26 million persons with severe disabilities at that time). Unfortunately, the information on employment rates is only available at the national level and cannot be used for the purposes of comparing Project NetWork sites. To compare employment rates among people with disabilities in the Project NetWork sites, we must turn to data from the 1990 Census (see Exhibit 3.4). These data provide information on employment rates among people reporting a disability, but, unlike, the SIPP, do not distinguish severe from nonsevere disabilities. Nevertheless, this provides a picture of the employment experiences of people with disabilities in the demonstration sites. Exhibit 3.4 presents employment rates for both persons who report having a disability and those who do not report a disability. The Census collects additional information from those who report having a disability. For those who are not in the labor force, the Census asks whether or not the individual is prevented from working by his or her disability. For example, the first

column of Exhibit 3.4 shows that in Dallas in 1990, 47 percent of those who had a disability were employed in 1990 and 6 percent were unemployed. Of the 47 percent who were not in the labor force, 38 percent reported being prevented from working by their disability, while 9 percent did not. The lower panel of the exhibit shows the employment status of other adults, those not reporting having a disability. In Dallas, 79 percent of other adults were employed, 5 percent were unemployed and 16 percent were not in the labor force.

Exhibit 3.4
Employment Status by Disability Status in the Project Network Service Areas in 1990

Percent of Population

	Dallas	Fort Worth	Phoenix/ Las Vegas	Minneapolis	New Hampshire	Richmond	Tampa	Spokane	U.S.
Employment Status of Those with Disabilities									
Employed	47	45	40	54	44	41	39	35	33
Unemployed	6	6	6	6	7	4	5	7	5
Not in Labor Force:[a]									
Prevented from working	38	40	45	31	40	45	48	47	52
Not prevented from working	9	9	9	9	9	10	8	11	10
Employment Status of Other Adults									
Employed	79	79	77	82	79	80	77	72	72
Unemployed	5	4	5	4	5	3	4	6	5
Not in labor force	16	17	18	14	16	17	19	22	23
Population size[b]	907,421	659,512	864,682	686,180	657,679	514,989	421,816	331,625	31,213,620

[a] Data indicating whether someone not in the labor force was prevented from working were only available for people with a work-related disability.
[b] Universe (N) = Civilian noninstitutionalized people age 16-24.

SOURCE: 1990 Census of Population and Housing Summary Tape File 3).

Comparing the demonstration sites to one another and to the nation, we find that in all sites, the percentage of persons with disabilities who were employed exceeded the national rate of 33 percent. The rate varied substantially among the demonstration sites from a low of 35 percent in Spokane/Coeur d'Alene to a high of 54 percent in Minneapolis. Minneapolis appears to offer a substantially different employment environment for persons with disabilities than in the other demonstration sites. Not only were over half of those with disabilities employed (compared with 47 percent in Dallas, the site with the next highest share), only 31 percent of those with disabilities who were not in the labor force reported that their disability prevented them from working. The latter rate varied from 31 percent in Minneapolis to a high of 48 percent in Tampa. Nationally, more than half of persons with disabilities who are not in the labor force say that their disability prevents them from working. While the employment rates of persons with disabilities

can be expected to depend on a variety of factors including individual characteristics (skills brought to the labor force, type and severity of disability) and strength of the local economy, the willingness of local employers to hire people with disabilities may also play an important role. It is possible that the variation in employment rates across the demonstration sites reflects some or all of these factors. These local environments may in turn contribute to the relative effectiveness of Project NetWork services in the demonstration sites.

3.2 Characteristics of the Treatment Group

Across all sites, a total of 4,160 individuals were assigned to the treatment group, comprising the total group of individuals to be served by the demonstration. As part of the demonstration design, SSA specified enrollment targets for each site; these targets were met or exceeded in all except one site. (The Fort Worth Model 1 site met 72 percent of its enrollment target.) Therefore, the actual size of the demonstration was nearly consistent with original plans. Models 2 and 3 were the largest demonstration models, with 1,088 and 1,087 treatment group members. The smallest was Model 1, with 956 treatment clients. Model 4 enrolled 1,029 in the treatment group. In this section we describe the demonstration intake and random assignment procedures and the characteristics of those assigned to the treatment group.

Random assignment assures that the treatment and control groups were alike at the time of volunteering for the demonstration with respect to characteristics that were or were not measured or may not even be measurable. Comparison of known characteristics of the participants confirms that the two groups, in fact, resembled one another closely. This section focuses on characteristics of the demonstration treatment group; a comparison of treatment and control group characteristics at baseline is provided in Appendix B.

3.2.1 Demonstration Intake and Random Assignment Procedures

The design of the demonstration featured several key components. As mentioned above, to ensure access to demonstration services for all who were interested, SSA established broad eligibility criteria. Individuals who expressed interest in the demonstration were interviewed by a Project NetWork case manager, who explained the requirements of the demonstration, the services being offered, and random assignment. To facilitate a rigorous analysis of demonstration impacts, all those who volunteered after this meeting were assigned at random to either a treatment group that received rehabilitation and employment services from the demonstration and special waivers of regular SSA requirements, or to a control group that received only the waivers, but no rehabilitation and employment services from the demonstration.[3] Control group members were eligible to participate in rehabilitation and employment services from other sources.

3 To ensure that the only difference between the treatment and control group members would be the receipt of Project NetWork services, SSA and ASPE decided to provide program waivers to both groups. These waivers increased the incentive to work by preventing disability benefit suspension or termination for all participants for at least one year during participation in the demonstration. The effects of these waiver provisions are assessed in a companion evaluation report.

Demonstration participants assigned to the treatment group began receiving employment and rehabilitation services from their case or referral manager almost immediately. The progress of clients was carefully monitored by the case and referral managers. Significant milestones — such as completing the intake interview, IEP/IRP, or job placement, and the purchase of rehabilitation services — were recorded in the automated MIS. Individuals could withdraw from the demonstration at any time, without sanction or loss of benefits.

3.2.2 Characteristics of the Treatment Group

Exhibit 3.5 displays the characteristics of the treatment group at the time of solicitation for the demonstration, using data from SSA administrative records. Overall, the treatment group, with an average age of 40 included a higher percentage of men (58 percent) than women (42 percent). Nearly half of all treatment group members were between the ages of 31 and 45 at solicitation. Among all treatment group members, 38 percent had graduated from high school, 19 percent attended some college, and 8 percent had completed a college degree. Data on years of education were missing for 14 percent of the treatment group.

The majority of treatment group members (42 percent) had a mental disability as their primary impairment. This category includes those with schizophrenia (found among 11 percent of the treatment group), psychoses and neuroses (24 percent) and mental retardation (8 percent). At the time of random assignment, 38 percent of the treatment group received SSDI benefits, 26 percent received SSI benefits and 13 percent received SSDI and SSI benefits concurrently. The remaining 23 percent were new SSI applicants at the time of random assignment, and therefore did not receive either type of benefits. We also examined the number of months that SSDI and SSI benefits had been received prior to random assignment. The treatment group had received an average of 36 months of SSDI benefits prior to random assignment and an average of 28 months of SSI benefits prior to random assignment. For those receiving SSDI at the time of random assignment, the average monthly benefit was $611. For those receiving SSI at random assignment, the average monthly benefit was $292.

Exhibit 3.5
Characteristics of the Project NetWork Treatment Group

Characteristic	Treatment Group	Characteristic	Treatment Group
Sample Size	*4,160*		
Demonstration Site		**Primary Impairment**	
Dallas	14%	Musculoskeletal	13%
Fort Worth	9	Neurological	6
Minneapolis	13	**Mental**	42
Phoenix/Las Vegas	13	Schizophrenia	11
New Hampshire	13	Psychoses and Neuroses	24
Richmond	13	Mental Retardation	8
Spokane/Coeur d'Alene	11	Other	32
Tampa	14	**Type of Disability Benefits received at Random Assignment**	
Gender			
Female	42	SSDI only	38
Male	58	SSI only	26
Age at Solicitation		Concurrent SSDI/SSI	13
18-30	22	New SSI Applicant	23
31-45	46	**Average number of months received benefits prior to Random Assignment**	
46-59	30		
60 and Over	3	SSDI	36
Mean Age	40	SSI	28
Race		**Average Monthly benefits at Random Assignment for those receiving:**	
Black	26		
White	65	SSDI	$611
Other	4	SSI	$292
Education			
<HS	21		
HS Graduate	38		
Some College	20		
College Degree	8		
Missing	14		

Source: SSA Administrative Records (SSR831, MBR831, MBR810/811 source files).

The baseline survey provides additional data on the characteristics of treatment clients, for the 1,311 individuals for whom an interview was completed. Exhibit 3.6 shows information from the survey on self-rated health conditions, functional limitations, and various work limiting conditions or situations.

Thirty-two percent of treatment group members rated their health as good, 13 percent as very good, and 8 percent as excellent. Among all treatment group members, nearly half (47 percent) rated their health as only fair or poor. The survey also asked respondents about the number of functional limitations they faced in their daily activities. Sixty-five percent of all treatment group members reported two or fewer functional limitations; 28 percent reported between three and four such limitations; and 6 percent reported having between five and seven limitations (26 percent reported no functional limitations).

Survey respondents were also asked if their primary disabling condition limited their ability to work. Seventy-seven percent responded affirmatively. However, only 21 percent responded that the condition prevented work altogether. Respondents were asked if they experienced transportation problems that limited work or other activities. Thirty-nine percent reported that transportation problems limited their work, and 38 percent said that transportation problems limited other activities.

Exhibit 3.6
Baseline Characteristics of Project NetWork Treatment Group with Completed Baseline Survey

Characteristic	All Models (N=1,311)
Self-Rated Health Condition	
Excellent	8%
Very Good	13
Good	32
Fair	32
Poor	15
Percent Reporting a Work Limiting Condition	77
Percent Reporting a Condition That Prevents Work	21
Number of Functional Limitations	
0	26
1-2	39
3-4	28
5-7	6
Transportation Problems	
Percent whole transportation problems limit work	39
Percent whose transportation problems limit other activities	38

SOURCE: Baseline Survey

Additional analysis of the baseline characteristics of treatment group members measured in the baseline survey is provided in Rupp et al. (1996). In particular, that analysis found that 35 percent of treatment group members reported ever having had to stay in a hospital for emotional problems, and 45 percent reported having felt depressed or sad much of the time in the previous year. Over half (56 percent) of the treatment group reported having spent between 1 and 90 days in deb during the past 12 months due to illness or injury. Rupp et al. Also examined the prevalence of depressive symptoms among the treatment group using the center for Epidemiological Studies depression screener (CES-D).[4] Using the CES-D, 46 percent of the treatment group were classified as depressed.

4 As reported in Rupp et al. (1996), the CES-D is widely used as a depression screener. Responses to 20 questions are scaled from zero to three based on the frequency of the self-reported presence of the feelings described by the given item (such as feeling sad or lonely), giving a range of possible scores from zero (least depressed) to 60 (most depressed). A value of 16 is often used as a cutoff, identifying those scoring 17 or higher as severely depressed. Using this cutoff, 46 percent of the treatment group were classified as depressed.

Chapter 4
Impacts on Employment and Earnings

The central objective of Project NetWork was to increase the employment and earnings of its participants. In this chapter, we determine whether that objective was achieved. We begin by briefly describing the estimation techniques and data employed in the analysis of impacts on employment and earnings. We then present our central findings—our estimates of impacts on earnings for the full sample. We briefly summarize estimated impacts on earnings (presented in Appendix C) for subgroups defined by site, disability program title, primary impairment, age, gender, and duration of prior receipt of benefits. We also present estimates of impacts on employment for the whole sample and then for key subgroups.

4.1 Analytic Approach

Impacts on employment and earnings, as well as the other outcomes analyzed in this report, were estimated by comparing the outcomes of the treatment and control groups. Because individuals who volunteered for the demonstration were randomly assigned to these two groups, any systematic difference in the subsequent outcomes of the two groups can be confidently attributed to the demonstration intervention. By "systematic difference," we mean differences that exceed what could be expected on the basis of chance alone; we use tests of statistical significance to determine the level of confidence we can have that the estimated impact represents a real effect, rather than a chance difference. In this analysis, any estimated impact that is larger than what could be expected on the basis of chance alone 90 percent of the time is deemed evidence of a real effect.

The results of these standard hypothesis tests should be interpreted with care. Whenever an estimated impact is not statistically significant, two explanations are generally possible. The first is that Project NetWork truly had no effect on earnings. The second is that Project NetWork really changed earnings, but the size of the true impact is too small to detect given the available sample size. It is especially important to keep these two interpretations in mind when examining estimated effects for subgroups with small sample sizes. Another potential problem with these tests, which are intended to identify effects which have of probability of less than 10 percent of occurring by chance alone, is that there is also a 10 percent chance that a single estimate will be statistically significant by chance alone, even when the true effect is zero. At least a few of these "false positives" are bound to appear whenever we examine a large number of impact estimates for many outcomes and subgroups.[1]

[1] These two types of errors are generally known as "Type I" and "Type II" error. "Type I" error (a "false positive") occurs whenever we reject the null hypothesis (that Project NetWork has no impact) when the null hypothesis is true. "Type II" error (often a problem of insufficient sample size) occurs whenever we accept the null hypothesis when it is false. As sample size grows, the minimum effect we can detect as statistically significant gets smaller.

The most important source of data on earnings is a set of administrative data on annual (calendar year) earnings from the Master Earnings File (MEF), provided by the Social Security Administration for all sample members. Earnings records from this source are available through calendar year 1996. Because impacts may vary according to time elapsed since random assignment, we converted these records of calendar year earnings to "follow-up year" earnings. We defined earnings in follow-up year 1 as earnings in the first full calendar year after random assignment, which occurred from mid-1992 through mid-1994. The "first follow-up year" is therefore calendar year 1993 for those randomly assigned in 1992; calendar year 1994 for those randomly assigned in 1993; and calendar year 1995 for those randomly assigned in 1994. Thus, our measure of average earnings in the "first follow-up year" includes some earnings obtained after the first twelve months following random assignment. All sample members have at least two follow-up years of earnings; those randomly assigned in 1992 and 1993 have a third follow-up year of earnings.

A second source of data on earnings and employment is the follow-up survey, which was conducted 25-36 months after random assignment. This follow-up survey attempted to re-interview a subgroup of persons interviewed for the baseline survey, which had achieved an 87 percent response rate. A total of 1,521 follow-up survey interviews were completed, for an 83 percent response rate. Thus, we completed both baseline and follow-up surveys with a 72 percent overall response rate.[2]

We employed slightly different methods to estimate impacts using the survey data and the administrative data. To protect the confidentiality of these data, all estimates of impacts on earnings using administrative data were performed by SSA/ORES staff. These estimates are based on simple comparisons of average earnings of treatment and control groups, without regression adjustment. Treatment-control differences in survey-measured outcomes have been adjusted by regression analysis to account for any chance differences in the measured baseline characteristics of the two groups, and to provide more precise impact estimates. A separate treatment group dummy for each site was included in these regressions to allow for intersite differences in impact. For the regression adjustment, baseline characteristics were measured with SSA administrative data from the MBR831, MBR810/811, and SSR831 source files, and from the in-person baseline interview. (See Appendix B for a complete description of the estimation procedures.)

In our analysis of earnings impacts, we place greater weight on estimates from the administrative data, because they are available for the entire sample of 8,248 persons, whereas follow-up survey data are only available for a subsample of 1,521; and because they are not subject to respondent

[2] The 72 percent figure is the product of 87 percent and 83 percent. See Appendix A for a more detailed description of the follow-up survey.

recall error and nonresponse, as survey data are. We do, however, provide the corresponding estimates from the survey data as corroborative evidence.[3]

Impacts on employment were also estimated from follow-up survey data. Our summary measure of employment is the total number of hours worked during each of four periods: the first year after random assignment, the second year after random assignment, the first and second years combined, and the 12 months prior to the follow-up interview. Given the timing of the follow-up interviews, the latter period ranged from months 13-24 to months 30-41 after random assignment. We also examined impacts on average number of months with any earnings.

4.2 Impacts on Annual Earnings of the Full Sample

The estimated impacts of Project NetWork on the earnings of the overall sample, based on administrative records data, are shown in Exhibit 4.1, by follow-up year. As can be seen, the demonstration increased the average earnings of the treatment group in the first two follow-up years by $215 and $224, respectively. These earnings gains represent increases of 12 percent and 11 percent over what treatment group members would have earned in the absence of the demonstration, as evidenced by the control group means. The average impact on annual earnings during these two follow-up years was a statistically significant $220, an 11 percent increase.

The estimated impact of Project NetWork on earnings in follow-up year 3 is, however, essentially zero. This estimated impact—the best evidence we have as to whether the effects of Project NetWork are long-lasting or temporary—can be estimated using only the 5,908 persons who were randomly assigned in 1992 or 1993. This estimate suggests that the effect of Project NetWork on earnings may be only a temporary one. As we will discuss in Chapter 6, our analysis of whether the benefits of Project NetWork (including increased earnings for participants) exceed the program's costs depends in part on our assumptions about the persistence of impacts on earnings beyond the observed follow-up period.

However, the observed decline in estimated impacts from the second to the third follow-up year is partly caused by the change in the composition of the available sample . The estimated impacts of Project NetWork on earnings (with asterisks denoting statistical significance) in follow-up years 1, 2, and 3 were, respectively, -$314, -$413, and -$329 for the 616 persons randomly assigned in 1992; $207*, $154, and $12 for the 5,292 persons randomly assigned in 1993, and $368* and $544*** (no year 3 results) for the 2,340 persons randomly assigned in 1994. Thus, estimated impacts were largest for those randomly assigned in 1994, for whom third-year impact estimates are not available. Had we been able to estimate third-year impacts with the full sample, including those randomly assigned in 1994, the estimated impacts may have remained statistically

[3]Any discrepancies in levels of earnings reported by the two data sources could exist because the two data sources may measure different types of earned income. Administrative data include SSA-covered earnings, but may exclude earnings from casual, informal jobs which respondents may report in the survey. Survey respondents may also forget to report earnings sources covered by the SSA administrative data.

significantly greater than zero in the third year. On the other hand, the estimated impacts for the large sample of persons randomly assigned in 1993 also declined in size from the first to the third follow-up year, a pattern of findings which suggests that program impacts do, in fact, decline over time. In sum, the evidence on the duration of impacts on earnings is somewhat ambiguous.[4]

Exhibit 4.1
Impacts on Annual Earnings, by Follow-up Year,
Based on Administrative Records—Full Sample

Follow-up Period	Average Annual Earnings, Control Group	Impact	Standard Error
Year 1	$1,757	$215**	$96
Year 2	2,106	224**	114
Year 1-2	1,931	220**	99
Year 3	2,427	-22	147

* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample Sizes:
For results in Year 1, Year 2, and Year 1-2: 4,160 in the treatment group, 4,088 in the control group, 8,248 in total.
For results in Year 3: 2,981 in the treatment group, 2,927 in the control group, 5,908 in total.

Earnings data are available for calendar years only, from 1990-1996. Random assignment occurred between mid-1992 to mid-1994. "Follow-up Year 1" is defined as the first full calendar year after the month of random assignment. For those randomly assigned in 1994, only 2 follow-up years of earnings data are available.

Earnings data are annual earnings data provided by SSA/ORES from the Master Earnings File (MEF).

These impacts are smaller in magnitude than those found in the Transitional Employment and Training Demonstration (TETD). In that test of employment services for SSI recipients with a diagnosis of mental retardation, an experimental evaluation found earnings gains of $714 per year, or 73 percent, over a six-year follow-up period.[5]

The estimated earnings impacts for the overall sample based on follow-up survey data were not statistically significant, and were somewhat smaller than the estimates based on administrative records (see Exhibit 4.2). Neither the difference in size nor the lack of statistical significance is surprising, given that the standard errors of these estimates are much larger than those for the

[4] The results also suggest the possibility that the program may have become more effective over time. According to the estimated impacts, the program caused negligible effects for those randomly assigned in 1992, some increase in earnings for those randomly assigned in 1993, and the largest increase in earnings for those randomly assigned in 1994. Random assignment occurred over two years (mid-1992 through mid-1994). Each site conducted random assignment over a 15-month period, beginning in June 1992 in Dallas and Fort Worth, with other sites beginning random assignment in early 1993 (Richmond was the last site to begin random assignment in March 1993). The program could have become more effective over time because the program became more effective within sites over time, or because sites that started later were more effective than sites that started earlier. It should be noted, however, that many site-specific factors could explain why the sites that started first could have had less effective programs than those that started later.

[5] See Thornton and Decker (1994).

estimates based on administrative records, because survey data are only available for a small portion of the sample.

Exhibit 4.2
Impacts on Average Annual Earnings,
Based on Follow-up Survey—Full Sample of Follow-up Survey Respondents

Follow-up Period	Average Annual Earnings, Control Group	Impact	Standard Error
Year 1	$1,074	-$72	$158
Year 2	1,740	162	220
Year 1-2	1,407	45	175
Latest Year	2,000	168	234

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample Sizes:
786 persons in the treatment group, 735 in the control group, 1,521 in total

Source: Follow-up Survey

Even though the estimated impacts on earnings based on administrative data are statistically significant, they are relatively small. The size of the average impact is not enough to make substantive improvements in the living standards of the average participant or to lift the average participant above the poverty line. It must be noted, however, that the estimates shown here are averages for the entire treatment group. Project NetWork may have led to substantially larger earnings gains for some participants and little or no gain for others--either because some individuals did not avail themselves of the services offered, because those services were inherently more effective for some sample members than for others, or because some service provision models were more effective than others. In an effort to identify subsets of the sample for whom Project NetWork services were particularly effective, the following sections of this chapter examine impacts on the earnings of subgroups defined by program model, disability title, primary impairment, age, gender, and duration of prior receipt of benefits.

4.3 Earnings Impacts, by Program Model

As noted at the outset, Project NetWork was implemented in eight sites, with four different models of service provision:

- the SSA Case Manager model (Model 1, Dallas and Fort Worth);
- the Private Contractor model (Model 2, Minneapolis and Phoenix/Las Vegas);

- the VR Outstationing model (Model 3, New Hampshire and Richmond); and,
- the SSA Referral Manager model (Model 4, Tampa and Spokane/Coeur d'Alene).

Appendix Exhibit C.1 shows the estimated impacts on average annual earnings, for follow-up years 1 and 2, for the four models and the eight sites. As Exhibit C.1 indicates, estimated impacts of Project NetWork on average earnings vary considerably across models and sites.

For many reasons, differences in estimated impacts across program models and sites are extremely difficult to interpret. The Project NetWork demonstration was not implemented as an ideal test of the relative effectiveness of the four program models. Observed differences in impacts across samples served by the four models reflect not only the relative effectiveness of the four program models, but also differences in the populations served, the availability of employment and training resources in the community, the local economy, the skills of local staff operating the program, and other factors. An additional problem is that the sample sizes for subgroups defined by site and model are much smaller than the total sample, so the minimum effect which can be detected as statistically significant is much larger than in the total sample, and the estimates are subject to much greater sampling variability.

The program produced statistically significant increases in earnings in only one site (New Hampshire) and statistically significant decreases in earnings in only one site (Spokane/Coeur d'Alene). The lack of statistical significance in the other sites may, however, simply reflect the small sample sizes available at the site level. An F-test for differences in impact across sites indicates that there were indeed significant differences in the site-specific impacts, but these could be due to any of the non-programmatic factors listed above. In short, we can say little with confidence on the basis of the site-specific results.

Similarly, estimated impacts by program model are statistically significantly different from one another, but the reasons for these differences are unclear. The program increased earnings by a statistically significant amount in the sample served by Model 3, and an F-test confirms that estimated impacts in the Model 3 sites are statistically significantly different from estimated impacts in the combined sample of Model 1 and Model 2 sites. This pattern of estimated impacts could have resulted from the many non-programmatic differences among the sites and participant subgroups associated with each model. Another possible reason for the observed impacts for the Model 3 subgroup is that the VR outstationing model may have served treatment group members by diverting resources from control group members, thereby biasing upward the estimated effects on earnings. Since demonstration staff in Model 3 were staff of the state VR programs, the primary alternative service provider for Project NetWork control group members, these staff may have felt that they had an incentive to divert resources from controls to treatments to make their program appear more effective. In the other sites, demonstration staff were not directly affiliated with state VR and therefore did not face this potential incentive. In sum, we can conclude that program impacts were largest in the Model 3 sites, but we cannot necessarily generalize from this

finding to conclude that a VR outstationing model will implement the program more effectively than the other models.

A possibly more concrete finding is that over follow-up years 1 and 2, the estimated impact on earnings for the Model 4 sample is statistically significantly smaller than the estimated impacts on earnings for the combined Model 1, 2, and 3 samples. The estimated impact on average annual earnings in follow-up years 1 and 2 for the combined Model 1, 2, and 3 samples is a statistically significant $338, while the estimated impact for the Model 4 samples is statistically insignificant from zero. Model 4—the Referral Manager model—is the least costly, least intensive program model, while the other three program models are designed to provide more intensive, higher cost services. We may be seeing evidence that the more intensive services are needed to produce a "payoff" of increases in earnings, but other interpretations of these results are also possible.

4.4 Earnings Impacts, by Title of Eligibility

The population eligible for Project NetWork—SSDI beneficiaries and SSI recipients and applicants—includes individuals in a wide variety of situations. It includes individuals who have just experienced the onset of disability and those who have been disabled for many years; the former may have very recent work experience, while the latter are likely not to have worked for some time. It also includes individuals in a broad range of financial circumstances, from those with incomes low enough to qualify for SSI to the generally better-off SSDI beneficiaries. These various groups may differ markedly in their ability and willingness to attempt to return to work. It is of interest, therefore, to examine whether the impact of Project NetWork differed by the title of eligibility of the participant. Distinguishing impacts by title of eligibility is also useful for policy purposes, since SSA could very well decide to adopt this intervention in one program and not the other, depending on its demonstrated effectiveness.

Exhibit 4.3 shows the estimated impacts on the annual earnings of the (mutually exclusive) subgroups of participants who, at the time of random assignment, received only SSI benefits, only SSDI benefits, both SSI and SSDI benefits, or neither type of benefit. The exhibit also shows estimated impacts for the subset of those who received neither type of benefit at random assignment and who were solicited for the demonstration as SSI applicants.[6] These persons did not receive benefits at the time of random assignment because their applications were rejected or pending, or because they received benefits for a brief period after solicitation but before random assignment.

[6] Those who received neither benefit at random assignment and who were not SSI applicants were recruited from other programs such as mental health services, or stopped receiving benefits between the time of solicitation and random assignment. We did not analyze program impacts for this group because the sample size was too small to analyze and because the group was less easily defined or replicated, and thus, impact findings may have had little policy relevance.

Exhibit 4.3
Impacts on Annual Earnings, by Title of Eligibility and Follow-up Year, Based on Administrative Records

Follow-up Period	Average Annual Earnings, Control Group	Impact	Standard Error
	Received only SSI at random assignment		
Year 1	$786	$190*	$100
Year 2	999	144	125
Years 1-2	893	167	104
	Received only SSDI at random assignment		
Year 1	$1,888	$296*	$173
Year 2	2,208	357*	205
Years 1-2	2,048	326*	179
	Received both SSI and SSDI at random assignment		
Year 1	$1,157	$41	$184
Year 2	1,401	-101	215
Years 1-2	1,279	-30	187
	Received neither SSI nor SSDI at random assignment		
Year 1	$2,997	$237	$251
Year 2	3,612	314	301
Years 1-2	3,305	275	259
Year 3	3,910	-15	369
	SSI Applicants		
Year 1	$3,132	$173	$271
Year 2	3,907	79	337
Years 1-2	3,519	126	285
F-test, difference in impacts among program subgroups			
Year 1		n.s	
Year 2		*	
Years 1-2		n.s.	

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

The sample sizes of persons analyzed for this exhibit are as follows:

Received SSI at random assignment: 1,096 in the treatment group, 1,064 in the control group, 2,160 in total
Received SSDI at random assignment: 1,570 in the treatment group, 1,556 in the control group, 3,136 in total
Received SSI and SSDI at random assignment: 553 in the treatment group, 539 in the control group, 1,092 in total
Received neither SSI nor SSDI at random assignment: 941 in the treatment group, 929 in the control group, 1,870 in total
Received neither SSI nor SSDI at random assignment, SSI applicants: 701 in the treatment group, 712 in the control group, 1,413 in total.

Earnings data are available for calendar years only, from 1990 to 1996. Random assignment occurred between mid-1992 and mid-1994. “Follow-up Year 1” is defined as the first full calendar year after the month of random assignment. For those randomly assigned in 1994, only two follow-up years of earnings data are available.

Earnings data are annual earnings data provided by SSA/ORES from the Master Earnings File (MEF).

As can be seen in the exhibit, Project NetWork produced somewhat larger estimated impacts on earnings among those who received SSDI at random assignment than among those in the other subgroups. For this group, Project NetWork increased average annual earnings by a statistically significant $326 during the first two follow-up years, a 16 percent gain. For those who received SSI at random assignment, Project NetWork increased earnings by a statistically significant $190, but only in the first follow-up year. The remaining estimated impacts for these subgroups were not statistically significant.

These results do not necessarily mean that Project NetWork had a greater effect on SSDI recipients than on the other sample members, even though we can assert that the program increased earnings for SSDI recipients. According to standard F-tests, we can reject the assumption that estimated impacts for the four title-of-eligibility subgroups are the same in follow-up year 2, but not in follow-up year 1 and not over the first two follow-up years combined. Another F-test indicates that the impacts on SSDI recipients were never statistically significantly different from the impacts on the combined sample of persons who either received SSI at random assignment or who received no benefits at random assignment. It is plausible that the program had a larger effect for this group because these persons already had some work experience, and perhaps, some may have needed only minor assistance to return to work. The combination of the program services and the demonstration waiver provisions could have been more effective for the SSDI recipients because the waivers removed larger work disincentives for SSDI recipients than for SSI recipients. According to these tests, we cannot, however, conclude that the program was more effective for SSDI recipients.

The numerous statistically insignificant effects in Exhibit 4.3 need not imply that there were no earnings gains for any of these subgroups or that there were no differences in impact among them. They simply mean that whatever impacts did occur were usually too small to be detected or distinguished with the sample sizes available for this analysis. The standard errors of most of these impact estimates are quite large, and confidence intervals around the estimated impacts include both positive and negative numbers that are large relative to the estimated impacts. For example, the 90 percent confidence interval around the estimated year 1-2 impact for the group that received only SSI at random assignment ranges from about -$50 to well over +$300. These subgroup samples are simply not large enough to allow precise estimation of effects of the size found in the overall sample.

The estimated impacts of Project NetWork on earnings, based on the survey data, are shown in Appendix Exhibit C.2. None of these estimated impacts on earnings are statistically significant. Because of the small sample sizes, the standard errors of these estimates are even larger than the standard errors obtained with administrative data.

4.5 Earnings Impacts, by Primary Impairment

The effectiveness of Project NetWork may also have varied with type of disability. We therefore estimated impacts on earnings by primary impairment, categorized as mental, neurological, musculoskeletal, or other. Exhibit 4.4 shows the results of the analysis of administrative earnings records for these subgroups.

In these data, the only subgroup for which earnings impacts were ever significantly different from zero was the subgroup whose primary impairment was other than mental, neurological, or musculoskeletal. This group showed an average annual earnings gain of $347 over the first two follow-up years, a 17 percent gain. Estimated impacts for those with mental impairments and musculoskeletal impairments also tended to be greater than zero but were statistically insignificant. An F-test indicates that the estimated impact for those with "other" impairments is not statistically significantly different from the estimated impact for the combined sample of those with mental and musculoskeletal impairments. Therefore, although we can have some confidence that the demonstration had a positive impact on the earnings of persons with "other" impairments, we cannot conclude that Project NetWork produced greater effects on this subgroup than on persons with mental or musculoskeletal impairments.

As in the other subgroup analyses presented in this chapter, the standard errors of estimate for the primary impairment subgroups are sufficiently large that only relatively large impacts could be detected as statistically significant; thus, these results do not rule out smaller impacts for the other three subgroups. For the same reason, the subgroup estimates were not significantly different from one another, even though they range from large positive numbers to large negative numbers.

Appendix Exhibit C.3 presents estimated impacts on earnings for these same impairment groups based on the survey data. The survey data indicate that Project NetWork also increased the earnings of persons with "other" impairments by a statistically significant amount in the latest observed follow-up year. The remaining impact estimates in Exhibit C.3 are generally statistically insignificant, an expected result given the limited sample sizes.

Exhibit 4.4
Impacts on Annual Earnings, by Primary Impairment and Follow-up Year, Based on Administrative Records

Follow-up Period	Average Annual Earnings, Control Group	Impact	Standard Error
	Mental Impairment		
Year 1	$1,547	$174	$124
Year 2	1,769	216	145
Years 1-2	1,658	195	126
	Neurological Impairment		
Year 1	$1,646	-$51	$395
Year 2	2,151	-499	457
Years 1-2	1,898	-275	406
	Musculoskeletal Impairment		
Year 1	$2,252	$9	$286
Year 2	2,759	235	365
Years 1-2	2,505	122	309
	2,960	165	496
	Other Impairments		
Year 1	$1,848	$363**	$171
Year 2	2,262	330	204
Years 1-2	2,055	347**	177
F-test, difference in impacts among disability subgroups			
Year 1		**	
Year 2		**	
Years 1-2		**	

Earnings data are available for calendar years only, from 1990 to 1996. Random assignment occurred between mid-1992 and mid-1994. "Follow-up Year 1" is defined as the first full calendar year after the month of random assignment. For those randomly assigned in 1994, only two follow-up years of earnings data are available.

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample Sizes:
Mental impairment: 1,762 in the treatment group, 1,735 in the control group, 3,497 in total
Neurological impairment: 252 in the treatment group, 223 in the control group, 475 in total
Musculoskeletal impairment: 534 in the treatment group, 486 in the control group, 1,020 in total
Other impairments: 1,612 in the treatment group, 1,644 in the control group, 3,256 in total

Earnings data are annual earnings data provided by SSA/ORES from the Master Earnings File (MEF).

4.6 Earnings Impacts, by Other Subgroups

In this section, we briefly summarize estimated impacts of Project NetWork on three other sets of subgroups defined by age, gender, and duration of prior receipt of benefits. Project NetWork may have different effects on these groups of persons, who may face different barriers to work. The findings are presented in Appendix C. We once again stress that these results should be interpreted with caution. Project NetWork may simply appear to have a statistically significant effect on some of these subgroups because age, gender, or duration of receipt may be correlated with other factors such as site and title of eligibility. The standard errors of estimates for many the subgroups are quite large, so that only relatively large impacts could be detected as statistically significant; thus, insignificant results do not rule out the presence of smaller impacts.

During the first two follow-up years, Project NetWork increased average annual earnings of men by a statistically significant $278, but did not increase average annual earnings of women by a statistically significant amount (Appendix Exhibit C.4). An F-test indicates that the estimated impacts for men and women are not statistically significantly different from one another. We can conclude that the program increased earnings for men, but we cannot conclude that the program had a larger effect on earnings for men than for women.

Estimated impacts on average annual earnings during follow-up years 1 and 2 for sample members aged 31-45 were a statistically significant $289 (Appendix Exhibit C.5). The estimated effects for those aged 46 and older were roughly the same size, but were not statistically significant from zero. Estimated effects for the 18-30 group were much smaller and statistically insignificant. An F-test shows that estimated impacts for the combined sample of those 31 and older were statistically significantly different from estimated impacts for those aged 18-30 at random assignment. These results suggest that younger persons may need additional services, or different services, to overcome barriers to work.

Finally, we examined the effect of the program on persons who received either SSI or SSDI at random assignment, further subdivided into two groups—those with less than three years of prior benefit receipt, and those with three or more years of prior benefit receipt, in order to focus on those who were clearly attached to either SSI or SSDI at random assignment. We dropped from the analysis those with no benefit receipt at random assignment, the vast majority of whom have less than 3 years of prior benefit receipt. As Exhibit C.6 indicates, estimated impacts for those with more months of prior benefit receipt were not statistically significantly different from those with fewer months of prior benefit receipt.

4.7 Impacts on Employment of the Overall Sample

Because of confidentiality restrictions with respect to access to individual earnings data, we were unable to provide estimates of Project NetWork's effects on employment using administrative records. The analysis of employment impacts is therefore based on data from the follow-up survey. We analyze impacts on total number of hours worked during each of four periods: the first year after random assignment, the second year after random assignment, the first and second years combined, and the 12 months prior to the follow-up interview. Given the timing of the follow-up interviews, the latter period ranged from months 13-24 to months 30-41 after random assignment. We also estimated impacts on average number of months employed during these same follow-up periods.

Exhibit 4.5 shows the estimated impacts on these outcomes for the overall sample. Although the estimated impact on total hours worked for the overall sample was not statistically significant, the demonstration did significantly increase the average number of months worked in three of the four periods analyzed, by 19 to 24 percent. Project NetWork could have increased average earnings and the average number of months employed, without increasing total hours worked, by increasing wage rates, but evidence presented in Section 4.9 indicates that wage rates did not increase. A more likely explanation is that total hours worked, which is estimated by multiplying reported weekly hours by the reported length of a job spell, is measured with more error than the number of months employed, which is based solely on the reported length of the job spells. In the following section, we briefly examine the employment effects on the other subgroups for which earnings impacts were estimated.

4.8 Impacts on Employment, by Title of Eligibility and Primary Impairment

Estimates of the demonstration's impacts on employment outcomes for subgroups defined by the type of disability benefits they were receiving at random assignment and by primary impairment are shown in Appendix Exhibits C.7 and C.8. These results are consistent with the estimated impacts on earnings for these two sets of subgroups based on the same data source. For those receiving SSDI at random assignment and those with "other impairments," Project NetWork increased the average number of months worked by a statistically significant amount in the second year and for the first and second years combined, although the size of this increase in both cases was only about one month per year. For none of the other subgroups are the estimated impacts on either of the employment outcomes consistently statistically significant across the four time periods.

Exhibit 4.5
Impacts on Employment—Full Sample of Follow-up Survey Respondents

Follow-up Period	Control Group Mean	Impact	Standard Error
	Total Hours Worked		
Year 1	191	-0	27
Year 2	312	32	38
Year 1-2	504	30	60
Latest Year	361	18	39
	Avg. # Months Employed		
Year 1	1.33	.22	.19
Year 2	2.14	.51**	.24
Year 1-2	3.47	.74*	.39
Latest Year	2.52	.49**	.25

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample Sizes:
786 persons in the treatment group, 735 in the control group, 1,521 in total

Source: Follow-up survey

4.9 Impacts on Hourly Wage Rates and Fringe Benefits

Two additional employment-related outcomes, hourly wage rates and the presence of fringe benefits on the current or most recent job, were measured in the follow-up survey. Estimated impacts on these outcomes are shown in Appendix Exhibits C.9, C.10, and C.11, for the sample overall and for some of the subgroups analyzed in earlier sections of this chapter. Note that those classified as having an hourly wage of $6.50 or below exclude those with no earnings.[7]

There were no statistically significant impacts on these outcomes for the sample as a whole. Among the subgroups analyzed, the only significant impacts on wage rates were increases in the proportion who earned *more* than $6.50 per hour among those with other impairments and those receiving neither SSI nor SSDI at random assignment; and an increase in the proportion who earned *less* than $6.50 per hour among those with mental impairments. The only subgroup to show consistent impacts on fringe benefits was those with neurological impairments, who

[7] For those who did not report wages on an hourly basis, the hourly wage was estimated with information on the earnings amount, the period for which earnings were reported (weekly, etc.), and hours worked.

experienced significant *reductions* in the incidence of three of the five fringe benefits analyzed.

It should be noted that, because these outcomes are only defined for those who are employed, impacts may reflect changes in the composition of the subgroup who were employed, as well as changes in the wages and fringe benefits that participants would have received in the absence of the demonstration. Thus, for example, an increase in the proportion earning more than $6.50 per hour could be due either to higher wage rates for those who would have been employed without Project NetWork or to an increase in the employment rate of higher wage workers.

4.10 A Caveat

Tests of statistical significance of the type used here identify those estimates that, *in a single trial*, have a probability of less than 10 percent of occurring by chance alone. This means, however, that there is a 10 percent chance that any given estimate *will* be statistically significant by chance alone, even when the true effect is zero. When large numbers of estimates are produced, we would expect one in ten to yield such a "false positive" result.

In this chapter, we have presented hundreds of impact estimates, of which only a small proportion were statistically significant at the 10 percent level. Therefore, it seems likely that some of the statistically significant impact estimates presented here are "false positives." Since there is no way to distinguish between the false positives and real effects, we can only urge some caution in the interpretation of these results.

4.11 Summary of Employment and Earnings Effects

In the analysis of Project NetWork's impacts on the annual earnings of the overall sample, based on administrative records, we found modest earnings gains that were statistically significant in the first and second follow-up years. Over these first two follow-up years, impacts on mean annual earnings averaged $220 per treatment group member, or 11 percent of the control mean. Because random assignment occurred over two years and we have earnings data for calendar years through 1996, only about 70 percent of sample members have a third year of follow-up data. For this limited sample, the estimated effect of Project NetWork on annual earnings declined to roughly zero in the third follow-up year. The overall impacts estimated from follow-up survey data were generally of the same magnitude, but were not statistically significant.

These findings suggest that Project NetWork's return-to-work services have successfully increased earned income. However, the increase in earnings may have been short-lived, and may have disappeared at roughly the time Project NetWork services ended. The size of the average impact, roughly $200 per year or roughly $20 per month, was not enough to increase the living standards of the average demonstration volunteer by a meaningful amount, and was not enough to lift the average demonstration volunteer above the poverty line. It is possible, however, that

Project NetWork could have produced very large earnings gains for a small group of demonstration participants.

In an effort to understand whether Project NetWork had larger effects on some persons in our sample, we estimated effects on earnings for several subgroups of interest. As we have stressed throughout this chapter, such an analysis of subgroup impacts requires caution. Whenever we analyze impacts for subgroups, the sample size declines. With smaller sample sizes, the standard errors of estimate for many of the subgroups become quite large, so that only large impacts could be detected as statistically significant. Finding statistically insignificant impacts need not rule out the presence of smaller impacts. Similarly, standard F-tests of the differences in impacts across subgroups are often a weak test of whether differences in impacts are present. At the same time, we have to be concerned about "false positives" because there is a 10 percent chance that any given estimate will be statistically significant by chance alone, even when the true effect is zero. Finally, even when we do find statistically significant impacts for subgroups, and we believe they are real effects, the interpretation of these findings is often unclear.

We could draw few strong conclusions from our analysis of impacts for samples in the eight research sites. Estimated gains in earnings were statistically significant in the New Hampshire site only. There was significant variation in estimated earnings impacts across sites, but this variation could be attributable to the type of program model implemented; the skill with which local staff implemented the program; the availability of local employment, training, and rehabilitation services; the characteristics of program participants; the local economy; and other factors. The estimated impacts were largest for the Model 3 sites (the VR outstationing model), but a similar range of explanations exists for this finding as well. An additional concern is that the estimated impacts for the Model 3 sites could have been biased upward because local staff may have reduced services available to control group members to serve the treatment group members. We found that estimated impacts on earnings were statistically significantly greater for the three more intensive, more costly program models (Models 1-3) than for the least intensive, least costly Referral Manager Model (Model 4). This finding suggests that increasing earnings for low income persons with disabilities requires more intensive services, but other interpretations of the evidence are also possible.

We also analyzed program impacts on earnings for subgroups defined by primary impairment and by title of eligibility at random assignment. We found that estimated impacts were statistically significant and largest for the subgroup who received SSDI at random assignment. It is possible that Project NetWork produces larger earnings gains for these persons because they have more work experience and perhaps need fewer services to return to work, and because the waiver provisions produce a stronger incentive to increase earnings for this group than for SSI recipients. On the other hand, the estimated impacts on earnings for this group were not statistically significantly different from estimated impacts for those who received SSI at random assignment or who were solicited as SSI applicants but did not receive benefits at random assignment. When we categorized sample members by primary impairment, we found that estimated impacts on

earnings were greater than zero and statistically significant for those whose primary impairment was other than mental, neurological, or musculoskeletal. However, estimated impacts for this group were not statistically significantly different from estimated impacts for those with mental or musculoskeletal impairments.

Estimated impacts on total hours of employment, as measured in the follow-up survey, were generally consistent with the earnings impacts derived from survey data and the administrative data. For the overall sample, we found increases of 19-24 percent in the average number of months worked in three of the four follow-up periods analyzed. Our overall conclusion is that Project NetWork successfully increased measures of work effort by statistically significant amounts. The next chapter explores whether this gain in earnings resulted in a decrease in reliance on SSI and SSDI benefits.

Chapter 5
Impacts on SSI and SSDI Benefit Receipt, and Measures of Health and Well-Being

The previous chapter showed that Project NetWork's case management services achieved their primary goal of increasing the earnings of at least some demonstration participants. An important potential consequence of earnings gains is the reduction in participants' transfer income, including income from SSI and SSDI benefits and other sources of assistance. This potential reduction in the average value of monthly SSI and SSDI payments would reduce the costs borne by the Social Security Administration, perhaps enough to offset the costs of NetWork services. Given the recent growth in the number of persons receiving disability benefits, it is of interest to know whether services of the type provided in the demonstration could lower caseloads and/or benefit costs. Another question is whether Project NetWork's services lead to improvements in attitudes and general health, thereby reducing the need for benefits. In this chapter, we explore the effect of Project NetWork on these outcomes.

Virtually all of the services supported by Project NetWork could lead to increased earnings and therefore to reduced benefit levels. Physical therapy and occupational rehabilitation could improve the health of participants and allow them to locate employment opportunities. Job placement and job search assistance help participants find employers who are willing to hire disabled persons. Assistance with education and training could help participants learn marketable skills desired by employers. Assistance with transportation and workplace facilities help persons retain their new jobs. Counseling could inform participants about how to obtain any of these services and provide support to participants as they make the transition to employment.

It is worth repeating that our analysis measures the incremental impact of Project NetWork's case management services beyond the "background" of services that persons in the treatment group would have received in the absence of the demonstration. Under the demonstration's random assignment design, the effect of Project NetWork on benefit receipt is estimated as the difference in average benefits between treatment and control group members. Members of the control group could also obtain counseling, therapy and rehabilitation, and employment and training services on their own, from other sources in the community.

We find that Project NetWork's services did *not* reduce participation in SSI and SSDI, and did not reduce average benefit levels for the full sample of randomly assigned persons. Furthermore, none of the four demonstration program models reduced benefit receipt, nor did the demonstration reduce benefit receipt among subgroups of persons defined by primary impairment or among subgroups receiving either SSI or SSDI in the month of random assignment. Project NetWork did, however, reduce benefit receipt over the follow-up period among persons who were solicited as SSI applicants and who received neither SSI nor SSDI at random assignment.

Despite this finding and despite occasional statistically significant effects on benefit receipt within some time periods and within some subgroups defined by site, type of impairment, and benefit receipt at random assignment, the effects of these services on benefit receipt were mostly minor and statistically insignificant. Project NetWork also had statistically insignificant effects on almost all survey-based measures of health and well-being, although the limited sample size of the survey sample prevents us from detecting any small but real effects on these outcomes. In the remainder of this chapter, we discuss these results in more detail.

5.1 Analysis methods for estimating impacts on benefit receipt

To adjust for any chance differences between treatment and control groups, and to improve the precision of our estimates, we have estimated impacts by employing standard regression adjustment to control for measurable characteristics defined at or before random assignment. We control for whether a person received SSI and/or SSDI in the month of random assignment, the value of SSI and SSDI benefits at random assignment, and the number of months each person had received SSI or SSDI benefits prior to the random assignment month. We also control for site, primary impairment, and demographic characteristics such as age, race, marital status, education, and other factors. These characteristics were obtained from several SSA administrative records. The reported impact estimates are averages of impacts within each of the eight research sites, with each site given equal weight. Appendix B provides additional details on our estimation procedures. Appendix D provides unadjusted means of important outcome measures analyzed in this chapter.

We focus mainly on two measures of benefit receipt. The first is the percentage of months within a specified follow-up period in which a person received SSI or SSDI benefits. The second is the average monthly value of SSI or SSDI benefits received within a specified follow-up period. In all exhibits, we show average benefits for all members of the subgroup analyzed, including those receiving no benefits.

By "follow-up period", we mean a specified number of months -- the first year, the second year, and so on -- after the month of random assignment. Because random assignment occurred between mid-1992 and mid-1994, these follow-up periods do not correspond to the same calendar periods for all sample members. The effect of Project NetWork could clearly vary over the length of the follow-up period. Some services, such as job placement and transportation assistance, could have an immediate effect on earnings. Other services, such as training and rehabilitation, could require years to affect earnings.

The monthly data on SSI and SSDI benefit receipt come from administrative data files provided by the Social Security Administration.[1] These data come from the administrative system that

1 The MBR810/811 and SSR831 are the source files for these data. The files are described in more detail in Appendix A.

processes benefits and are therefore extremely reliable measures of benefit receipt. Because we have monthly data on SSI benefit receipt through the end of calendar year 1996, and monthly data on SSDI benefit receipt through the end of calendar year 1997, we have at least 30 months of data on SSI benefit receipt and 42 months data on SSDI benefit receipt for all randomly assigned persons. The monthly benefit values are expressed in terms of 1996 dollars.

5.2 Impacts on Benefit Receipt for the Full Sample

As Exhibit 5.1 indicates, for the full sample, NetWork had a negligible, statistically insignificant impact on all measures of benefit receipt over the follow-up period. The point estimates of impact are never more than about one percent of the control group mean values. In addition, there are no time trends in these impact estimates: in each follow-up period, NetWork had a negligible effect. Over the available followup periods, 36 percent of control group members received SSI benefits in an average month, 51 percent of control group members received SSDI benefits in an average month, the average SSI benefit of control group members was $111, and the average SSDI benefit of control group members was $308.[2] The corresponding percentages and dollar amounts for treatment group members were virtually identical.

It should be noted that the waiver provisions, which applied to both treatment and control groups, may have prevented Project NetWork services from reducing SSDI benefits for at least two years after random assignment. The waivers, which were intended to remove strong work disincentives in the SSDI program, were activated in the first month in which earnings exceeded $200 or self-employment exceeded 40 hours (the same criterion used to determine a Trial Work Period (TWP) month[3]). Once in effect, the waiver continued for the next 12 months regardless of subsequent employment. For SSDI beneficiaries in this waiver period, no month could be counted as part of the TWP, or result in benefit interruption for those who were in the extended period of eligibility. After the waiver period ended, earnings gains for this group did not affect benefits for up to another year (the 9-month TWP plus a 3-month grace period). For those SSDI beneficiaries, then, increases in earnings would not result in benefit reductions until at least two years after random assignment. Because most beneficiaries did not earn any income at the time of random assignment, even more than two years would be needed for most persons to find jobs, exhaust the waiver period, Trial Work Period and grace period, and then begin to lose benefits as a result of earnings.

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2 These average benefit amounts are defined for the whole sample or subgroup analyzed, and include zero values for nonrecipients.

3 The Trial Work Period (TWP), one of the standard work incentive provisions in the SSDI program was enacted as part of the Social Security Amendments of 1960 (P.L. 86-778). Each month in which earnings from work exceed $200 or self-employment exceeds 40 hours is counted as a TWP month. The TWP provision allows SSDI beneficiaries to have a total of nine such months during a rolling period covering the most recent five years. During the TWP benefits are unaffected by earnings. At the end of the TWP, a determination is made concerning the beneficiary's ability to sustain earnings at the substantial gainful activity (SGA) level. If earnings are lower than SGA levels (i.e., $500 per month), regular SSDI eligibility is continued. If earnings have consistently exceeded the level of SGA during the TWP, cash benefits are then continued during a three-month grace period, and the beneficiary simultaneously enters the 36-month Extended Period of Eligibility (EPE).

Exhibit 5.1
Impacts on Benefit Receipt, by Follow-up Period-- Full Sample

Follow-up Period	Control Group	Impact	Standard Error
	Percentage of months receiving SSI		
Months 1-12	37.1	0.1	0.4
Months 13-24	34.9	0.0	0.6
Months 25-30	33.8	-0.1	0.6
Months 1-30	35.5	0.0	0.5
	Percentage of months receiving SSDI.		
Months 1-12	52.8	-0.4	0.3
Months 13-24	51.5	-0.5	0.5
Months 25-30	49.6	-0.5	0.6
Months 31-42	47.7	-0.6	0.6
Months 1-42	50.5	-0.5	0.4
	Average monthly SSI benefits		
Months 1-12	$117	-0	$1
Months 13-24	$108	-0	$2
Months 25-30	$104	-2	$2
Months 1-30	$111	-1	$2
	Average monthly SSDI benefits		
Months 1-12	$323	-$3	$2
Months 13-24	$315	-$4	$3
Months 25-30	$302	-$3	$4
Months 31-42	$290	-$4	$5
Months 1-42	$308	-$3	$4

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
4,160 persons in the treatment group, 4,088 in the control group, 8,248 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

It is also possible that the waivers do not explain the absence of effects on benefit receipt. The demonstration's modest impact on average annual earnings— discussed in the previous chapter—may not have been large enough to cause a substantial impact on benefit receipt, even in the absence of the waivers. The estimated impact of the program on average annual earnings was $220—roughly $18 per month. The waivers had no effect on the many participants whose earnings were not increased by the program. The program may have increased earnings for some SSDI beneficiaries by an amount which was simply not large enough to have increased the number of TWP months, even in the absence of the waiver. Therefore, for the participants whose earnings increased under the program by a small amount per month, the waiver may not have been needed to maintain eligibility for benefits. Another possible explanation for these findings is that the program mainly affected earnings of sample members who had left assistance.

The waivers for SSI recipients were less likely to delay the effects of the demonstration on benefits. In SSI, the waivers prevented special disability or blindness reviews that could normally occur when earnings exceeded $500 per month. But these reviews were seldom conducted during the demonstration period in any case, and the waivers had no effect on the SSI "tax rate" on earnings, which immediately reduced benefits by $.50 for every $1.00 of earnings above $65 per month under the regular program rules. For SSI recipients, Project NetWork may have had no effect on benefit receipt because the program mainly affected earnings of sample members who had left assistance, or because the program's effect on earnings for those who continued to receive SSI was simply too small to have caused a statistically significant reduction in benefits.

5.3 Impacts on Benefit Receipt by Title of Eligibility

The negligible overall estimated impacts on the full sample could mean that Project NetWork did not affect benefit receipt of any person, or that it affected benefit receipt of some persons in a way that is "buried" in an analysis of average outcomes for the entire sample. We begin our analysis of impacts on subgroups by asking whether Project NetWork had different impacts for persons who received SSI only, SSDI only, both SSI and SSDI (concurrent recipients), or neither benefit in the month of random assignment.

These four groups could have very different characteristics and face very different circumstances. SSDI recipients must have prior work experience to qualify for SSDI benefits, while SSI recipients have relatively less work attachment. In the demonstration, those receiving neither benefit at random assignment include several groups who may be very different from ongoing SSI and SSDI participants. Some were new SSI applicants recruited by Project NetWork. These new applicants received no benefit at random assignment because their applications either were still pending or had been denied. Those receiving neither benefit at random assignment also include persons recruited from other programs, such as mental health services, and persons who had received either SSI or SSDI just before random assignment and then left these programs. The estimates of impacts on benefit receipt by these "title of eligibility" subgroups are shown in Exhibit 5.2.

The most striking finding in Exhibit 5.2 is that Project NetWork's services reduced all measures of benefit receipt by small, but statistically significant amounts among those who received neither type of benefit at random assignment. Over the available follow-up period, about 5.4 percent of control group members in this subgroup, but only 3.7 percent of treatment group members, received SSI benefits in a typical month. Project NetWork therefore reduced SSI participation by 1.7 percentage points among this subgroup, statistically significant at the 5 percent level. Similarly, Project NetWork reduced the percentage of persons receiving SSDI benefits from 4.9 percent to 3.1 percent, a statistically significant reduction. These impacts, while not large, were very consistent over the follow-up period. They appeared immediately during the first 12 follow-up months, and then rose somewhat over time. This pattern of impacts also persisted within the

Exhibit 5.2
Impacts on Benefit Receipt, by Title of Eligibility

Follow-up Period	SSI			SSDI Only			Both SSI and SSDI			Neither SSI or SSDI			Neither SSI or SSDI at Random Assignment, SSI Applications		
	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error
Percentage of months receiving SSI															
Months 1-12	92.0	-.4	.9	2.4	.4	.5	83.9	1.8	1.6	3.5	-1.1*	.6	3.6	-1.6**	.7
Months 13-24	84.2	.1	1.3	4.1	-.2	.6	73.2	3.6*	2.2	6.3	-1.9**	.9	6.3	-2.3**	1.1
Months 25-30	81.9	-.5	1.5	3.7	.4	.7	69.4	3.3	2.4	7.1	-2.4**	1.1	6.9	-2.5**	1.2
Months 1-30	86.8	-.2	1.1	3.3	.2	.5	76.7	2.8	1.7	5.4	-1.7**	.8	5.3	-2.0**	0.9
Percentage of months receiving SSDI.															
Months 1-12	5.8	-.5	.8	98.3	-.5	.4	99.2	.1	.4	2.7	-1.0*	.6	2.3	-.8	.5
Months 13-24	7.7	-.6	1.0	94.0	-.9	.8	95.1	1.3	1.0	4.6	-1.5*	.8	4.4	-1.6*	.9
Months 25-30	8.1	-.7	1.1	88.9	-.2	1.1	92.2	1.1	1.5	6.0	-2.3***	.9	5.7	-2.3**	1.0
Months 31-42	8.2	-.5	1.1	84.9	-1.0	1.2	87.3	2.7	1.8	6.8	-2.8***	.9	6.7	-2.9***	1.1
Months 1-42	7.4	-.5	.9	91.9	-.7	.7	93.6	1.3	1.0	4.9	-1.8***	.7	4.6	-1.9**	.8
Average monthly SSI benefits															
Months 1-12	$364	$-5	$4	$3	$2**	$1	$133	$4	$5	$12	$-5**	$2	$12	$-6**	$3
Months 13-24	333	-3	6	5	2	1	109	6	6	21	-9***	3	21	-9**	4
Months 25-30	322	-5	7	4	2	1	95	0	5	24	-10**	4	24	-9**	5
Months 1-30	343	-4	5	4	2*	1	116	4	5	18	-8***	3	18	-8***	3
Average monthly SSDI benefits															
Months 1-12	$35	$-5	$5	$675	$-3	$3	$395	$-0	$3	$21	$-7	$5	$16	$-5	$4
Months 13-24	44	-7	6	646	-5	6	384	3	6	31	-9	6	27	-8	6
Months 25-30	45	-7	6	609	0	8	373	6	8	37	-12**	6	33	-10	7
Months 31-42	45	-5	6	580	0	11	361	1	11	40	-14**	6	37	-13*	7
Months 1-42	42	-6	6	628	2	8	383	-5	8	31	-10*	5	27	-9	6

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level
Sample sizes:
Received SSI at random assignment: 1,096 in the treatment group, 1,064 in the control group, 2,160 in total
Received SSDI at random assignment: 1,570 in the treatment group, 1,556 in the control group, 3,136 in total
Received SSI and SSDI at random assignment: 553 in the treatment group, 539 in the control group, 1,092 in total
Received Neither SSI nor SSDI at random assignment: 941 in the treatment group, 929 in the control group, 1,870 in total
SSI applicants: 701 in the treatment group, 712 in the control group, 1,413 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

smaller subgroup of persons who received neither type of benefit at random assignment, and who were solicited for the demonstration as SSI applicants.[4]

For the group without benefits at random assignment, NetWork also reduced average monthly SSI benefits by a statistically significant $8 and reduced average monthly SSDI benefits by a statistically significant $10 over the available follow-up periods.[5] These reductions in average benefits, while small in terms of dollars per month, amount to 30-43 percent of average benefits of control group members in this subgroup. These effects again persisted within the smaller subgroup of persons who received neither type of benefit at random assignment, and who were solicited as SSI applicants.

The estimated impacts on measures of benefit receipt for the other three subgroups were mostly much smaller and were insignificantly different from zero. Only one of these impact estimates achieved statistical significance for the overall follow-up period: among those receiving only SSDI benefits at random assignment, NetWork reduced average monthly SSI payments by $2, from $6 to $4 on average.

It is somewhat surprising that Project NetWork, a demonstration offering case management and referral services for SSI recipients and SSDI beneficiaries, had its largest effect on SSI and SSDI benefit receipt among SSI applicants who were not receiving benefits initially, yet generally no impacts on groups with much more lasting attachment with the SSI and SSDI programs. We can think of two possible explanations. The first is an "entry effect: " Project NetWork's case management and referral services helped participants find services so they could avoid relying on SSI and SSDI benefits later. The second is a "deterrence" effect: some treatment group members incorrectly thought they had to participate in unwanted services to obtain SSI or SSDI — a not unreasonable assumption given the increasing work requirements in cash assistance and Food Stamp programs— and chose to give up these benefits.

The results in Exhibit 5.2 also show that benefit receipt in the month of random assignment is a reliable predictor of benefit receipt over the followup period. Of those who received only SSI at random assignment, 87 percent received SSI in a typical follow-up month, but only about 7 percent received SSDI. Similarly, of those who received only SSDI at random assignment, 92 percent received SSI in a typical follow-up month, but only about 3 percent received SSI. Among concurrent beneficiaries at random assignment, average SSI and SSDI participation rates were 77 and 94 percent, respectively. Among those who received neither benefit at random assignment, the average monthly participation rates were 3-5 percent. These patterns suggest that the four

4 The sample of persons who received neither type of benefit at random assignment, and who were not solicited for the demonstration as SSI applicants, include persons who were solicited from other programs, such as mental health services. These persons are not analyzed separately because they do not constitute a clearly defined subgroup, and the demonstration was not designed to measure the effects of Project NetWork on them.

5 The impacts for those with neither benefit at random assignment were also statistically significantly different from impacts for the rest of the sample members (based on an F-test).

title of eligibility subgroups accurately define 4 very different subgroups of persons, each with very different patterns of benefit receipt and circumstances.

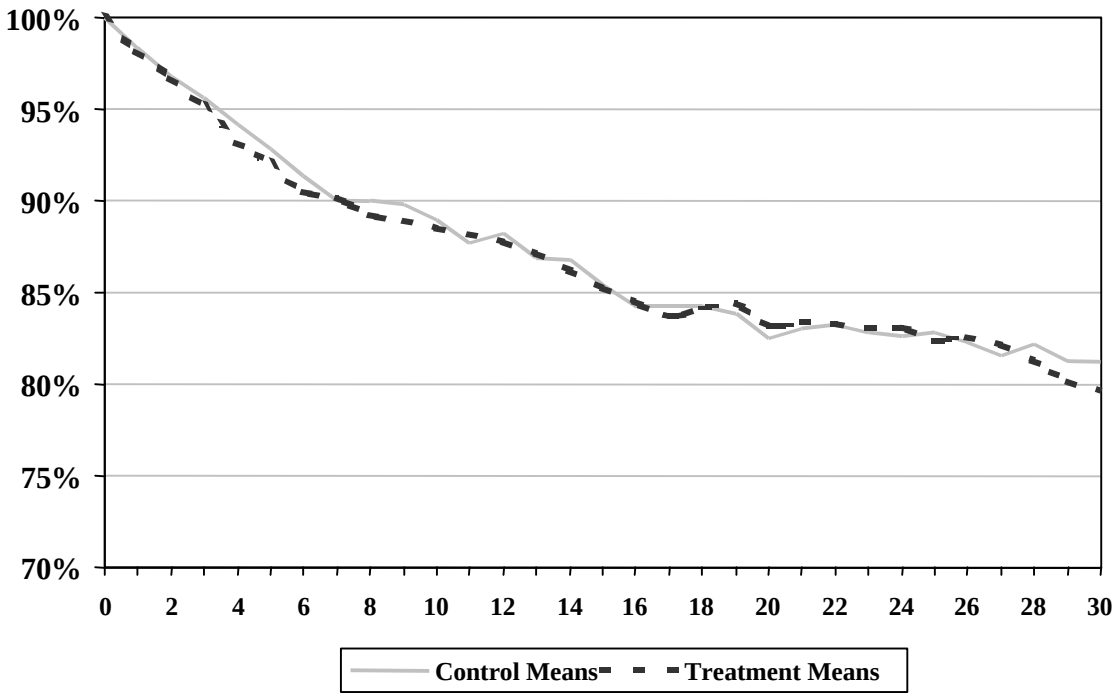
The percentage of persons receiving either SSI or SSDI declined very slowly over the 30- or 42-month follow-up period. This finding is shown more clearly in Exhibit 5.3, a series of line graphs of month-by-month participation rates in SSI and SSDI for treatment and control group members. In these graphs, the monthly participation rates for each type of benefit are estimated using the sample receiving that benefit at random assignment. After 30 months, about 80 percent of persons who received SSI at random assignment continued to receive SSI. After 42 months, about 82 percent of persons who received SSDI at random assignment continued to receive SSDI. Those who left SSI or SSDI were not necessarily working. Many may have received support from relatives or other sources of assistance, and about 7 percent of the full sample had their benefits terminated due to death.[6] The numeric results corresponding to these graphs are displayed in Appendix exhibits D.1, D.2, D.3, and D.4.

Project NetWork also had no statistically significant impacts on the duration of spells or on measures of recidivism. This finding is not surprising, given that impacts on rates of benefit receipt were generally negligible and given that so few participants who originally received benefits left assistance over the follow-up period. About 4 percent of treatment group members left SSDI for at least 3 months and then returned to SSDI, and about 8 percent of treatment group members left SSI for at least 3 months and then returned to SSI. The percentages for control group members were virtually identical.[7] Most participants were either always receiving benefits or never receiving benefits.

6 We have only limited information on reasons for benefit termination other than death. The available SSDI benefit data on terminated benefits during the follow-up period did not indicate whether a person left SSDI because of earnings or for some other reason. The available SSI data provide more detailed information on the reasons for termination. Most SSI recipients who leave the program do so for reasons other than increases in earnings. In general, however, the "benefit termination codes" in administrative data are often unreliable because the recipient leaves the program regardless of the reason provided, so there is little incentive to provide accurate information.

7 These findings are not shown in Exhibits.

Exhibit 5.3
Impacts on SSI Participation Rates - Those Receiving only SSI at Random Assignment



Impacts on SSDI Participation Rates - Those receiving only SSDI at Random Assignment

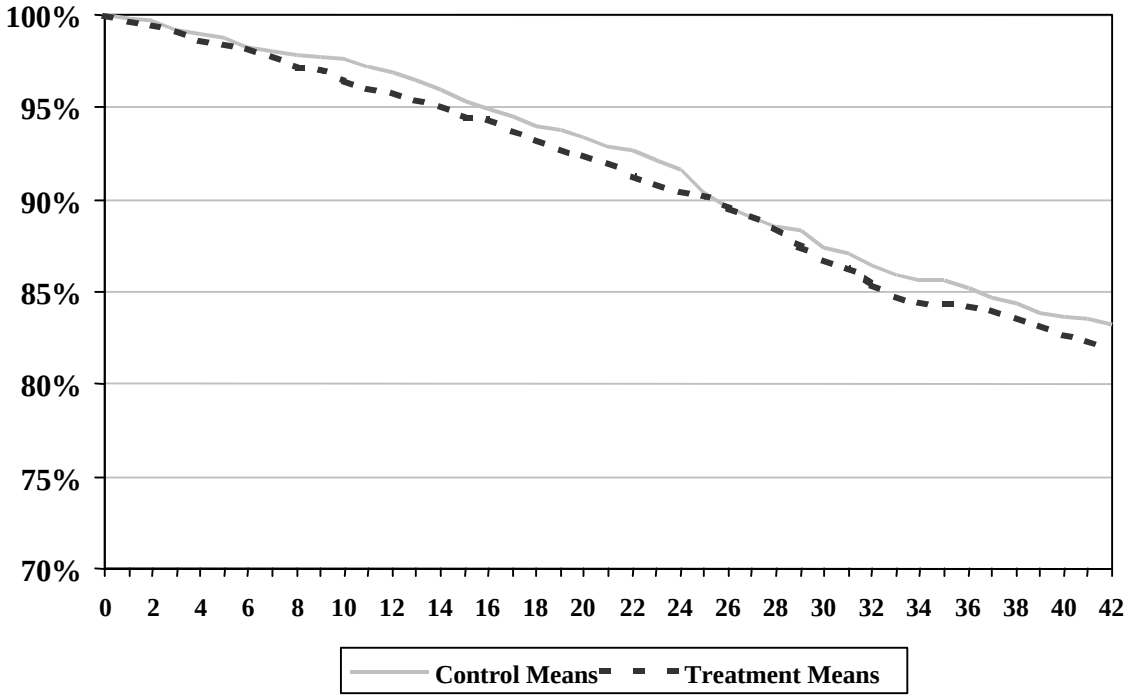
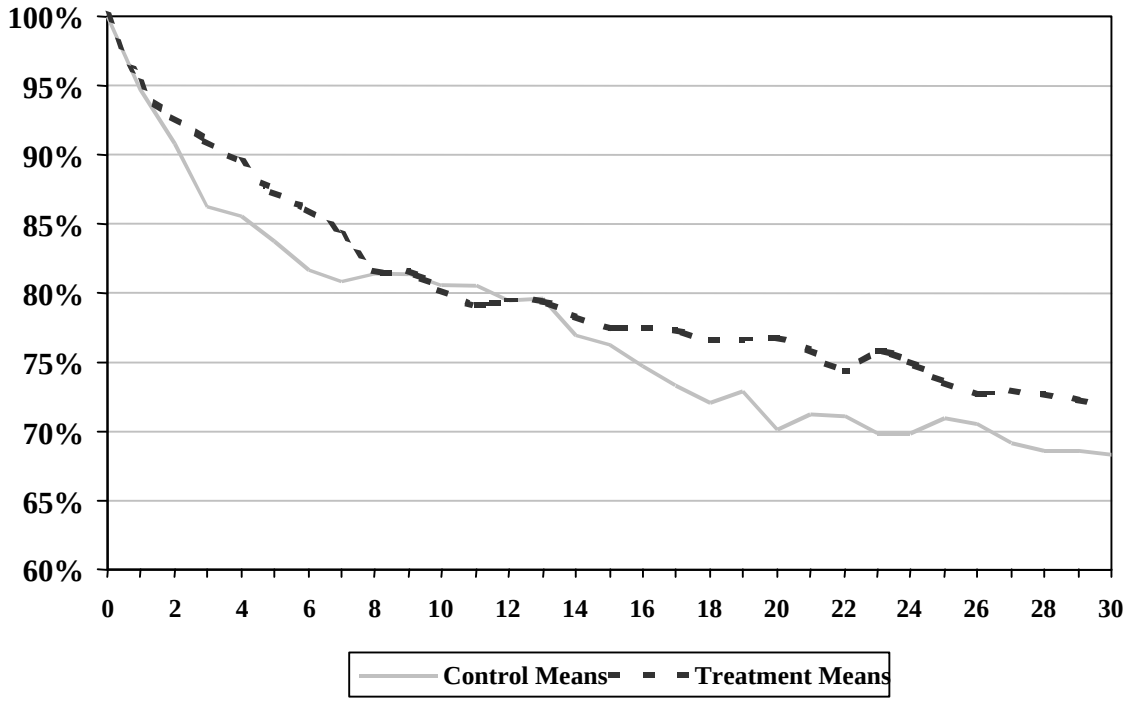
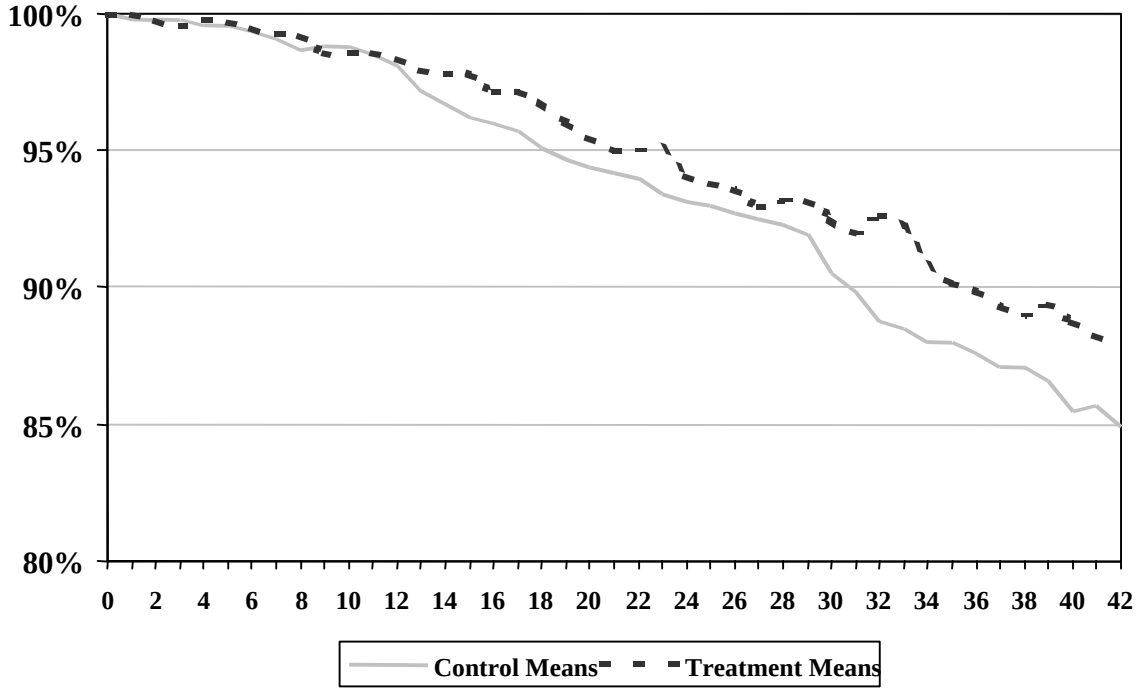


Exhibit 5.3
Impacts on SSI Participation Rates - Those Receiving Both SSI and SSDI at Random Assignment



Impacts on SSDI Participation Rates - Those Receiving Both SSI and SSDI at Random Assignment



5.4 Impacts on Benefit Receipt by Program Model

Project NetWork was implemented by four different program models, and each of the four different program models operated in two sites:

- the SSA Case Manager model (Model 1) in Dallas and Fort Worth;
- the Private Contractor model (Model 2) in Phoenix/Las Vegas and Minneapolis;
- the VR Outstationing model (Model 3) in New Hampshire and Richmond, Virginia; and,
- the SSA Referral Manager model (Model 4) in Tampa and Spokane/Coeur d'Alene.

Appendix exhibits D.5 through D.8 show the estimated impacts on benefit receipt, for the entire available follow-up period, for each of these four models and eight sites. As these exhibits show, both the impact estimates and the average values of outcomes for the control group vary considerably across sites and models.

For many reasons, differences in estimated impacts across program models and sites are extremely difficult to interpret. The Project NetWork demonstration was not implemented as an ideal test of the relative effectiveness of the four program models. Observed differences in impacts across samples served by the four models reflect not only the relative effectiveness of the four program models, but also differences in the populations served, the availability of employment and training resources in the community, the local economy, the skills of local staff operating the program, and other factors. Moreover, since the sample sizes for subgroups defined by site and model are much smaller than the total sample, the minimum effect which can be detected as statistically significant is much larger than in the total sample, and the estimates are subject to much greater sampling variability.

None of the four program models reduced benefit receipt by consistent, statistically significant amounts. Project NetWork achieved statistically significant reductions in the percentage of months of receipt of SSI in only one site -- New Hampshire. In New Hampshire, the demonstration also reduced average SSI benefits by $8 per month. In Richmond, the demonstration increased the percentage of months of SSI receipt by a statistically significant 2.1 percentage points. In the Phoenix/Las Vegas site, Project NetWork reduced average SSDI benefit levels per month by a statistically significant $20. The other estimated impacts were not significantly different from zero.

The simplest interpretation of these results is that none of the 4 models was consistently effective. As noted earlier, impacts on earnings need not necessarily translate into effects on benefits, because of the waivers provided by the demonstration, and because, even without the waivers, the effects on average earnings may simply have been too small to have reduced benefits. It is also true, however, that the sample sizes available for this analysis are only sufficient to detect moderately large impacts on benefits. Because each model served roughly

one-quarter of the entire sample of 8,248 demonstration participants, estimated impacts within each model subgroup would have to be about twice as large as in the full sample to attain a given level of statistical significance. Estimated impacts on monthly benefit receipt within each model subgroup would typically have to be $10 or more per month to attain statistical significance at the ten percent level.

5.5 Impacts on Benefit Receipt by Type of Primary Impairment

Persons with different types of impairment face different barriers to employment and self-sufficiency. Accordingly, we examined the impacts of Project NetWork on benefit receipt among persons grouped by primary impairment. As in the analysis of the impacts on earnings, we have grouped the sample according to four major types of impairment:

- Mental impairments, defined as psychoses and neuroses, schizophrenia, and mental retardation;
- Neurological impairments, which are diseases of the central nervous system;
- Musculoskeletal impairments; and,
- Other impairments.[8]

Exhibit D.9 summarizes the impact of Project NetWork on benefit receipt within each of these subgroups. The demonstration generally did not produce statistically significant impacts on measures of benefit receipt within these primary impairment subgroups. The only statistically significant effect over the follow-up period as a whole was among those with impairments affecting the musculoskeletal system, where Project NetWork reduced the percentage of persons receiving SSDI benefits by an average of 2.1 percentage points (4 percent); this reduction averaged about 3 percentage points during the third follow-up year.

5.6 Impacts on Benefit Receipt for Other Subgroups

In this section, we summarize estimated impacts of the demonstration on three other sets of subgroups defined by age, gender, and duration of prior receipt of benefits. Project NetWork may have different effects on these groups of persons, who may face different barriers to work. The findings are presented in Appendix D. We once again stress that these results should be interpreted with caution. Project NetWork may simply appear to have different effects on some of these subgroups because age, gender, or duration of receipt may be correlated with other factors such as site and title of eligibility. The standard errors of estimates for many of the

8 These includes infectious and parasitic diseases, neoplasms, endocrine and metabolic disorders, complications of pregnancy, disorders of the skin and subcutaneous tissue, congenital abnormalities, perinatal diseases, and diseases of the blood and blood forming organs, eye, ear, circulatory system, respiratory system, digestive system, and genitourinary system.

subgroups are quite large, so that only relatively large impacts could be detected as statistically significant; thus, insignificant results do not rule out the presence of smaller impacts.

In general, we find that impacts do vary across these subgroups, but the impacts remain generally small, and the reasons for this variation in effects are unclear. During the first 42 follow-up months, Project NetWork reduced the percentage of months of SSDI receipt by a statistically significant 1.1 percentage points among women, but not among men. (Appendix Exhibit D.10.) During follow-up months 31-42, the demonstration also reduced average SSDI benefits for women by a statistically significant $11, but did not have a statistically significant effect on average SSDI benefits for men. F-tests indicate that these estimated impacts for men and women are statistically significantly different from one another. These impacts are not readily explained by program impacts on earnings, which were larger for men (although by a statistically insignificant amount.)

Estimated impacts on benefit receipt for subgroups defined by age at random assignment were statistically insignificant, with two exceptions. The program reduced average monthly SSDI benefits in follow-up months 31-42 by a statistically significant $13 among those age 18-30 at random assignment, and reduced average monthly SSDI benefits in follow-up months 1-12 by a statistically significant $8 among those age 46 and older at random assignment. (Appendix Exhibit D.11) F-tests show that differences in estimated impacts on these outcomes for the three age groups were statistically significant. However, the program did not produce consistent, statistically significant effects on benefit receipt for any of three groups.

Finally, we examined the effect of the program on persons who received either SSI or SSDI at random assignment, further subdivided into two groups—those with less than 3 years of prior benefit receipt, and those with 3 or more years of prior benefit receipt, in order to focus on those who were clearly attached to either SSI or SSDI at random assignment. We dropped from the analysis those with no benefit receipt at random assignment, the vast majority of whom had less than 3 years of prior benefit receipt. As Exhibit D.12 indicates, Project NetWork reduced the percentage of months of SSDI receipt and average SSDI benefit levels by statistically significant amounts during several follow-up periods for those with less than 3 years of prior receipt of benefits. For example, during the second follow-up year, Project NetWork reduced the average percentage of months of SSDI receipt by 2 percentage points, and average monthly SSDI benefits by $15 for this subgroup. These reductions in SSDI receipt did not occur for those who had received benefits for three or more years prior to random assignment. In fact, during follow-up months 25-30, the demonstration increased average monthly SSDI benefits for those with more prior dependence by a statistically significant $10.

5.7 Impacts on Measures of Health and Well-Being

The primary goal of Project NetWork was to increase earnings and employment and reduce reliance on SSI and SSDI benefits. Chapter 4 and the previous sections of this chapter have shown that Project NetWork's case and referral management services increased average earnings but did not change the rate of receipt of SSI and SSDI benefits in the full sample of persons who volunteered for these services. In this section we examine the impacts of the demonstration on other outcomes of interest, including health and functional status, and general attitudes and outlook. Project NetWork may have strengthened the mental health of participants, thereby producing an indirect positive effect on employment (with respect to willingness to work or to continue working). We examine the impact of Project NetWork on self-reported measures of health and well-being provided by the 1,521 follow-up survey respondents.

Project NetWork helped participants obtain several types of services that might improve health and well-being. Physical therapy and occupational therapy could directly improve the health and fitness of participants. Psychological counseling could help participants gain a more positive outlook, and to take the steps needed to obtain and retain jobs. The general counseling and assistance offered by the case/referral managers could improve participant-esteem as well as help them consider new options for employment.

For many reasons, however, the effect of Project NetWork on the health of participants is likely to be modest at best. Although average earnings increased under NetWork, the vast majority of participants continued to receive SSI or SSDI benefits. Project NetWork did increase the rate at which participants received services, but nevertheless, as reported in Chapter 2, a large percentage of control group members obtained similar services on their own.

We find that Project NetWork's case and referral management services generally did not have statistically significant effects on the measures of health and well-being collected in the followup survey. Project NetWork did increase by about 5 percentage points the proportion of respondents who stated that they were better off at the interview date than a year before. This impact, however, was not corroborated by improvements in more objective measures of health and well-being. The limitations of this analysis should be kept in mind. In particular, because of the limited available sample of survey respondents, we can only detect fairly large effects as statistically significant; smaller but genuine impacts will be statistically insignificant from zero.

The measures of health and well-being we use are based on respondents' answers to a series of questions in the follow-up survey. These questions pertain to respondents' self-assessed health, disabilities, work limitations, and cognitive and emotional state. The meaning of many questions is less clear-cut than questions about earnings, employment, and benefit receipt. Phrases such as "good health," and " difficulty hearing or speaking" could mean different things to different respondents. It seems likely that at least some respondents may not have provided candid answers to some of the more personal questions, especially those about the use of alcohol and drugs.

While such reporting errors may bias the *levels* of the outcome variables, they should not bias the impact estimates unless there is differential reporting error between the treatment and control groups. These reporting errors will, however, increase the variance of the outcome measures and thus increase the size of impact estimates we can detect as statistically significant. To improve the precision of the impact estimates, we use regression adjustment to control for baseline characteristics obtained from both administrative and survey data. These baseline variables include the same measures of health and well-being, obtained from the baseline survey. The estimated impacts of Project NetWork on these measures of health and well-being are shown in Exhibit D.13.

Measures of overall health. The survey asked whether the respondent's health at the interview date was, in general, "excellent, very good, good, fair, or poor," and whether the respondent's current health had gotten "worse, stayed the same, or improved" since random assignment. Project NetWork had no statistically significant impact on these self-reported measures of health. Only about 17 percent of treatment group members rated their health as excellent or very good, and only 20 percent said their health had improved since random assignment.

Functional and life skills limitations. A substantial proportion of treatment group members had three or more functional or life skills limitations. About 44 percent of treatment group members reported having some difficulty with activities such as seeing words in a newspaper, hearing an ordinary conversation, having speech understood, lifting 10 pounds objects such as grocery bags, walking up a flight of stairs without resting, walking 3 city blocks without resting, or using a telephone. About 29 percent of treatment group members had difficulty with routine activities around the house: getting around inside or outside the home, getting out of bed or out of a chair, taking a bath or a shower, dressing, eating, using the toilet, keeping track of money, preparing meals, or doing light housework. The services provided by Project NetWork did not have a significant impact on the rate at which respondents reported these difficulties.

Use of alcohol and drugs. Roughly 11 percent of treatment group members reported drinking excessively since the time of random assignment, while another 15 of treatment group members admitted that they use drugs to get high since random assignment. Project NetWork did not affect the rate at which this behavior was reported.

Measures of emotional state. Project NetWork had mixed effects on these measures. About 15 percent of treatment group members had to stay overnight in a hospital because of emotional problems, and almost two-thirds felt sad, blue, or depressed for at least two weeks or more over the previous year. Project NetWork had no effects on these outcomes, nor did it have any impact on respondents' average scores on the Mental Health Inventory. This test is a subset of the 38-item Mental Health Inventory used in the Health Insurance Experiment to measure mental health

status.[9] Project NetWork did, however, increase by about 5 percentage points the percentage of respondents who reported being better off today than a year ago, and who felt things would be better a year from now.

Mini Mental State Examination. The Mini Mental State Examination (MMSE) is used as a screener for cognitive impairment. Cognitive impairment refers to problems in the performance of such cognitive abilities as attention, memory, language, calculation, orientation, and reasoning. The MMSE was developed by Folstein, Folstein, and McHugh (1975) and was included in both the Project NetWork baseline and follow-up surveys. Respondents are asked to state the current date and geographic location, repeat some words read by the interviewer, spell "world" backwards, recall some words said a few seconds earlier, identify the names of simple objects such as a pencil, fold a piece of paper, write a sentence, and copy a simple figure. Respondents receive points for each correct response, with a perfect score being 30. The average score of both treatment and control group members was 27; thus, Project NetWork had no detectable impact on this measure. In both groups, about 30 percent received perfect scores, and about 90 percent received at least 25 points.

Measures of work limitations. A large number of Project NetWork participants reported limitations in obtaining and maintaining employment. About 44 percent of treatment group members said that an illness or injury kept them in bed for at least 7 days during the previous 12 months. About 35 percent said their health condition prevented them from working at all, and 27 percent said their disability prevented full-time work. Over 40 percent reported transportation problems which limit their ability to work. Despite the fact that Project NetWork increased earnings, it did not have a statistically significant impact on these self-reported measures of work limitations.

Impacts by subgroup. We examined impacts on these outcomes across the subgroups defined by program model, title of eligibility at random assignment, and primary impairment. The findings did not differ in systematic ways across these subgroups. As a result, a summary of impacts for the full survey sample is sufficient to describe the effects of Project NetWork on these measures of health- and well-being.

5.8 Conclusion

In this chapter, we have analyzed the impact of Project NetWork's case management services on receipt of SSI and SSDI benefits, and on a number of measures of health and well-being. We

9 Berwick *et al*. (1991). The questions are: Have you been a very nervous person? Have you felt calm and peaceful, to downhearted and blue? Have you been a happy person? Have you felt so down in the dumps that nothing could cheer you up? Respondents could answer "all of the time, most of the time, a good bit of the time, a little of the time, or none of the time."

measured benefit receipt over the period after random assignment by the percentage of persons receiving SSI or SSDI in an average month and by average monthly SSI and SSDI benefits. We could follow receipt of SSI for 30 months and receipt of SSDI for 42 months. We constructed several measures of heath and well-being from responses to the client survey. Our main findings were as follows:

- Project NetWork did not affect measures of benefit receipt in the full sample of demonstration participants by statistically significant amounts.

- Project NetWork consistently reduced all measures of benefit receipt by statistically significant amounts among persons who received neither SSI nor SSDI in the month of random assignment, and who were solicited for the demonstration as SSI applicants. These impacts were fairly small: the program reduced the average percentage of months of SSDI and SSI receipt by roughly 1-3 percentage points per month, and average monthly SSDI and SSI benefits by $5-$13. Project NetWork generally had no statistically significant impacts on benefit receipt among subgroups who received SSI or SSDI or both in the month of random assignment.

- The estimated impacts on each of the four program models were generally statistically insignificant as well. Across the eight demonstration sites, estimated impacts on benefit receipt were mostly statistically insignificant, and varied for reasons which were unclear.

- Project NetWork reduced the percentage of persons with musculoskeletal impairments who received SSDI benefits in an average month by about 2 percentage points (4 percent), but generally had no other statistically significant impacts on benefit receipt within subgroups defined by primary impairment.

- Project NetWork had no statistically significant impacts on a variety of measures of health and well-being, although it did increase the percentage of persons who reported that they were better off than a year ago, and the percentage who expected things to be better a year later.

Despite these generally negligible effects on SSI and SSDI benefit receipt, Project NetWork did increase earnings. At least three explanations are possible for this combination of findings. Demonstration services may have increased earnings by large amounts among the small proportion of persons who had left SSI and SSDI.[10] Or, demonstration services may have

10 This combination of results—increased earnings with no significant effect on transfer benefits—has arisen in a number of studies of training programs for welfare recipients (see, e.g., Gueron and Pauly, 1991; Friedlander and Burtless, 1995; and Orr *et al.*, 1997). In the one case where the explanation could be reasonably determined, (AFDC recipients enrolled in JTPA), it seemed clear that the earnings impacts were confined to recipients who had left the welfare rolls (and who would have left the rolls in the absence of the program); see Orr *et al.*, 1997. In addition, this combination of results is consistent with the Transitional Employment Training Demonstration conducted by

increased earnings among the larger proportion of participants who remained on SSI or SSDI and the waiver provisions allowed them to continue to receive benefits while their earnings increased over much of the available followup period. A third explanation is that the program increased earnings among those who continued to receive benefits, but the size of the increase — roughly $18 per month— was typically too small to affect benefits.

Even with the aid of the generous case and referral management services provided by Project NetWork, reducing reliance on SSI and SSDI benefits for these persons over a 30-42-month follow-up period is obviously a challenging task. The problems faced by persons with disabilities often last many years, if not a lifetime, and the average spells of SSI and SSDI receipt are far longer than the available follow-up period for this study. The measures of health and well-being clearly show that substantial proportions of NetWork participants still face serious barriers to work. Given the long-term difficulties facing SSI and SSDI recipients, it may be reasonable to expect Project NetWork's services to help persons increase earnings while they continue to receive benefits over this follow-up period.

It is worth repeating the limitations of our analyses of differences in impacts across subgroups. The minimum detectible impact rises as we examine subgroups consisting of fewer persons, so estimated impacts must be larger to attain statistical significance and be distinguishable from chance impacts. This problem of limited sample size is especially important in the analysis of measures of health and well-being, based on the sample of survey respondents. Even if impacts within subgroups do attain statistical significance, if we find a small number of significant impacts after we examine a very large number of subgroups, some of these may be "false positives." Since the impact estimate is likely to be statistically significant by chance alone 10 percent of the time, if we perform this test across 10 subgroups, we would expect to find one statistically significant impact even if the program has no real effects. Among the very large number of impact estimates analyzed in this chapter, a small number were statistically significant, and these were often scattered across subgroups and time periods with no clear pattern. Even if we believe that occasional statistically significant estimated impacts within specific subgroups are real effects, the meaning of these impacts is often unclear because the role of program model, site-level factors, personal characteristics, and other factors are hard to separate.

Despite these limitations, our general conclusion is that services like those provided by Project NetWork will not reduce overall SSI and SSDI caseloads or benefits by substantial amounts. This conclusion seems especially clear when we recall that only about 5 percent of the eligible SSI and SSDI caseload volunteered to participate in Project NetWork, so the impacts of the demonstration on benefit receipt of the entire SSI and SSDI caseload are far smaller than the impact estimates presented here. The next chapter presents our benefit-cost analysis of Project NetWork, and ties together our estimates of impacts on benefit receipt, service receipt, and earnings.

SSA in the mid-1980s (see Thornton and Decker 1994).

Chapter 6
Costs and Benefits of the Project NetWork Demonstration

This chapter assesses Project NetWork's costs and benefits in order to address several important policy questions: Is the disabled population eligible for Project NetWork better off financially as a result of the program? From the standpoint of the Social Security Administration, how do the SSI and SSDI benefit reductions generated by the demonstration program compare to its costs? Taking these benefit reductions and costs into account, as well as the program's effect on state vocational rehabilitation expenses and federal and state tax payments, what is the financial impact of the Project NetWork demonstration on taxpayers? Finally, combining these perspectives, what are the benefits and costs of Project NetWork to society as a whole?

The chapter begins by explaining the approach used in this benefit-cost analysis and then presents estimates of the program's direct and indirect costs. Next, the analysis develops estimates of Project NetWork's benefits reflecting the program's impacts on earnings, SSI and SSDI benefits, and other outcomes discussed in the last two chapters. Last, the estimated benefits and costs of the program are assessed from the perspectives of disabled persons, the Social Security Administration, state and local government, taxpayers, and society. This assessment considers the four Project NetWork program models as well as the overall program.

6.1 Analytical Approach

The main task of this benefit-cost analysis is placing dollar values on Project NetWork's net effects and net use of resources. Thus, it estimates the program's benefits and costs, per treatment group member, minus the benefits and costs that would have occurred in the absence of the program (based on the experience of the control group). The program's effects on a number of outcomes are derived from the impact estimates presented in Chapters 4 and 5.[1] Effects on other outcomes are estimated as treatment-control differences, based on data from the follow-up survey and other sources.

[1] Point estimates of program impacts are used in this analysis regardless of whether the impacts were found to be statistically significant.

Net resource use is measured as the difference in resource use between the treatment and control groups. The treatment group's use of Project NetWork resources is measured using program expenditure and participation data, and both groups' use of non-NetWork resources is estimated using the survey data and valued using state vocational rehabilitation agency cost data on pertinent services.

In a demonstration program such as Project NetWork, most expenditures are made in the first year or two following random assignment, while many program effects last for a much longer time. As a result, the observation period for this analysis fully captures Project NetWork's resource use, but not all its effects. Our estimates of program effects on SSI payments, for example, cover only 30 months. In order to indicate how the benefit-cost results would change if Project NetWork's future effects are taken into account, the analysis projects such effects beyond the observation period using assumptions about how observed impacts change in the longer run. Some of the conclusions of this analysis of Project NetWork turn out to be sensitive to these assumptions.

Once estimated, particular components of the analysis constitute benefits or costs (or neither) depending on the analytic perspective taken. The perspectives used in the Project NetWork benefit-cost analysis are shown in Exhibit 6.1. In the exhibit, a plus sign indicates that an item is expected to be a benefit from a particular perspective; items that are expected to be costs from that perspective are identified with minus signs and items that are expected to be neither benefits nor costs are denoted by zeros.

The perspective of disabled persons identifies gains and losses to members of the treatment group. By taking into account impacts on earnings, SSI and SSDI payments, and other pertinent program effects, this perspective essentially shows how their net incomes change as a result of the program.

The perspective of the Social Security Administration counts benefits and costs of Project NetWork to the agency that funded the demonstration. From this perspective, reductions in SSI and SSDI payments, which are a cost from an individual's perspective, are important benefits. Direct expenditures on the Project NetWork demonstration, which were all incurred by the agency, constitute the key cost component from the SSA perspective.

When the perspective of the Social Security Administration is combined with the "Other Federal" perspective, we obtain the federal government's overall point of view. Thus, for example, while SSI and SSDI payment impacts and Project NetWork expenses are counted from the SSA perspective, impacts on Food Stamps, Medicaid, and federal income taxes affect other branches of the federal government.

Exhibit 6.1
Expected Value Of Components Of The Benefit-Cost Analysis, By Accounting Perspective

Component	Disabled Persons	Social Security Administration	Other Federal Government	State and Local Government	Society
Reduced use of transfer programs					
SSI and SSDI payments	-	+	0	+	0
Food stamps	-	0	+	0	0
Medicaid payments	-	0	+	+	0
Unemployment insurance	+	0	+	+	0
SSI and SSDI administration	0	+	0	+	+
Food stamp administration	0	0	+	+	+
Medicaid administration	0	0	+	+	+
UI administration	0	0	+	+	+
Increased earnings and fringe benefits	+	0	0	0	+
Increased tax payments					
Federal income tax	-	0	+	0	0
State income and sales taxes	-	0	0	+	0
Social security tax	-	+	0	0	0
NetWork Program Costs					
Assessment services	0	-	0	0	-
Employment and training services	0	-	0	0	-
Medical treatment services	0	-	0	0	-
Other purchased services	0	-	0	0	-
Site operations	0	-	0	0	-
Central administration	0	-	0	0	-
Non NetWork Costs					
Physical therapy	0	0	0	-	-
Counseling	0	0	-	-	-
Training and life skills	0	0	-	-	-
Assessment	0	0	0	-	-
Occupational therapy	0	0	0	-	-
Job search assistance	0	0	-	-	-
Business skills training	0	0	-	-	-
College classes	0	0	0	-	-
Other job-related training	0	0	-	-	-
Other services	0	0	-	-	-
Participant Costs					
Out of pocket expenses	-	0	0	0	-
Time lost to participation	-	0	0	0	-

The perspective of state and local government captures Project NetWork's impacts on state income taxes, Medicaid expenditures, state vocational rehabilitation agency costs, and other pertinent outcomes. It also takes account of state supplements of federal SSI payments, which occurred at all Project NetWork sites except Dallas and Fort Worth (Texas does not supplement federal payments). Combining the SSA, other federal government, and state and local government perspectives yields an overall budget or "taxpayer" point of view that counts benefits and costs to everyone in society other than the treatment group.

The last perspective, that of society as a whole, combines those of disabled persons and taxpayers (the latter combines the SSA, other federal, and state and local government perspectives). For any component of the analysis, there is a net gain to society if the gain to one group (disabled persons or taxpayers) exceeds the loss to the other. Thus, one can add the estimated value for disabled persons to the value for taxpayers in order to arrive at the net value to society.

All benefit and cost results in this analysis are expressed in 1994 dollars, the year in which most program costs were incurred. Program effects and resource use that occurred before, during, and after that year are adjusted to reflect their value in 1994. This is done using a real annual discount rate of five percent, which takes account of forgone investment as well as inflation. The difference between the net benefit and net cost estimates – the program's net present value – is then calculated from each of the analytic perspectives. This is the measure of overall program effectiveness that is used in the analysis.

6.2 Resource Use

Project NetWork was a federally funded, locally operated demonstration program. The program's direct expenditures, incurred by the Social Security Administration, were for services and assistance provided solely to members of the treatment group. In addition, disabled persons in both the treatment and the control groups could obtain services that were not paid for by the Social Security Administration. These non-NetWork costs must also be included in the analysis because the program could increase or decrease the use of these services by the treatment group compared with the control group.

6.2.1 Project NetWork Expenditures

As shown in Exhibit 6.2, the average total Project NetWork expenditures per treatment group member was $3,660, which includes $2,397 for site operations, $212 for central administration, and $1,051 for four types of purchased services. These various components of Project NetWork's direct costs were estimated in two steps. The first step is to calculate Project NetWork program participation measures for the treatment group. For the site operations and central administration components – which include case management, direct services provided by Project NetWork staff, other program operations, and site and central

management – the participation measure is the treatment group's average length of participation in Project NetWork, measured in months. For purchased services, the measure is the proportion of the treatment group that received each of four types of services: assessment, medical treatment, employment services, and other services.

These Project NetWork participation measures were then multiplied by a unit cost – that is, the average cost of providing services of a given type to one person. For each of the four purchased services, the numerator of the unit cost estimate is the total cost to Project NetWork of that type of service, as measured by vendor payments recorded by the program across all sites. The denominator is the number of Project NetWork participants who, according to program MIS data, received that particular service at least once. For site operations and central administration, total Project NetWork staff and nonpersonnel expenses are allocated between these two components and then divided by the total number of Project NetWork participation months recorded for the treatment group. Further details on the measurement of Project NetWork costs are provided in Appendix E.

Exhibit 6.2
Direct Costs per Treatment Group Member, by Component

Component	Average Direct NetWork Cost per Treatment Group Member
Purchased Assessment Services	$264
Purchased Employment & Training Services	625
Purchased Medical Treatment	63
Other Purchased Services	99
Site Operations	2,397
Central Administration	212
Total	$3,660

The resulting estimates of Project NetWork's direct costs are shown in Exhibit 6.2. Across the sample as a whole, site operations, including case management and direct provision of services, accounted for more than 60 percent of the direct costs. The bulk of these Project NetWork site expenditures consisted of site personnel costs. About a third of the direct Project NetWork costs were for purchased services, with employment and training services accounting for most of these expenses.

Direct Project NetWork costs were considerably higher for some sites than others. The highest costs, in Richmond ($5,305 per treatment group member) and Dallas ($4,326), were more than twice the average direct cost in Tampa ($2,129) and Spokane ($2,180), which relied primarily on referrals to other programs that provided services at no cost to the

demonstration, rather than direct service provision. Site-specific direct costs are discussed further in Appendix E.

6.2.2 Non-NetWork Expenditures

The need for the evaluation to fully capture total resource use by the Project NetWork demonstration means that the analysis must take into consideration expenditures on the treatment group made by all outside organizations, regardless of whether Project NetWork reimbursed these expenditures. In some cases, the receipt of such services was the result of referrals and other arrangements made by site staff. In other instances, Project NetWork participants found these services on their own.

The control group, of course, was also entitled to receive non-NetWork services. Thus, it was necessary to measure non-NetWork costs for both the program and the control groups. The first step in doing this was to measure service receipt for the two groups, using survey data. For the treatment group, services that survey respondents said they did not receive from Project NetWork were counted as non-NetWork services. For the control group, all services reported by respondents were counted as non-NetWork services. See Appendix E for further discussion of the use of survey data in estimating non-NetWork service utilization.

The second step was estimating unit costs to apply to these participation estimates. These unit cost estimates are all based on state vocational rehabilitation agency expenditures and aggregate service use during 1994 in the states where the Project NetWork demonstration sites operated. Appendix E provides a description of the state vocational rehabilitation data that were used in making these estimates as well as an explanation of how the estimates were derived.

As shown in Exhibit 6.3, the estimated non-NetWork expenditures on the control group were considerably higher than for the treatment group. As a result, there is an estimated non-NetWork cost ***saving*** of $1,453 per treatment group member, which offsets about 40 percent of the direct cost of the Project NetWork demonstration. Most of this estimated saving reflects the higher use of non-NetWork physical therapy, counseling, job search assistance and other job-related training services by control group members, along with the non-NetWork assistance with transportation and other needs that they received. The non-NetWork expenditures varied considerably across the eight demonstration sites, as discussed in Appendix E.

The cost of non-NetWork services to the treatment group is estimated to be $326 per treatment group member in the full sample. This means that the full cost of the services received by the treatment group was $3,986, including $3,660 in direct Project NetWork costs and $326 for non-NetWork expenditures. This is the ***gross*** cost of the Project NetWork treatment.

Exhibit 6.3
Non-NetWork Costs per Treatment Group Member, by Type of Service

Type of Service	Average Cost per Treatment Group Member	Average Cost per Control Group Member	Net Non-NetWork Cost saving per Treatment Group Member
	Full Sample		
Physical Therapy	$24	$332	$308
Counseling	52	571	519
Training and Life Skills	23	84	61
Assessment	31	52	21
Occupational Therapy	8	64	56
Job Search Assistance	72	148	76
Business Skills Training	62	91	29
College Classes	17	73	57
Other Job-Related Training	29	144	115
Other Services	9	26	17
Other Assistance	0	194	194
All Services	$326	$1,779	$1,453

The cost of non-NetWork services to the control group averaged $1,779 per control group member. This means that the ***net cost*** of the Project NetWork treatment was $3,986 (the gross cost of services provided to treatment group members) minus $1,779 (the cost of the services they would have received in the absence of Project NetWork), which is $2,207. The net cost for Richmond, $3,826 per treatment member, was the highest among the eight demonstration sites. In contrast, the net costs for the Spokane and Tampa sites were less than $1,000. These were the two sites that relied primarily on referrals to other programs, rather than direct service provision. See Appendix E for detailed estimates of net costs by site.

6.3 Program Effects on Earnings, SSI and SSDI Benefits, and Related Outcomes

The analysis of earnings impacts presented in Chapter 4 showed that Project NetWork led to significant impacts on the work and earnings of the full sample during the first two years of follow-up, but had no effect in the third year for the portion of the sample for which three years of follow-up data were available. As indicated in Chapter 5, the overall effects on SSI and SSDI benefit receipt were small and statistically insignificant. However, these measured differences were relatively consistent over the follow-up period.

Project NetWork's estimated impacts on these outcomes were used to estimate the dollar value of a number of the benefits and costs listed in Exhibit 6.1, namely earnings, fringe benefits, SSI payments, SSDI payments, sales and excise taxes, income and payroll taxes, and SSI and SSDI administrative costs. We base our estimates of earnings-related effects on the impact estimates derived from administrative data, because they are available for the entire sample and cover a longer follow-up period than the survey data. The administrative data may also be more accurate, because they are reported by employers, close to the time of payment and are therefore not subject to nonresponse and recall error. The other program effects listed in the exhibit were estimated using survey data.

6.3.1 Earnings

Exhibit 6.4 presents the net present value of earnings gains over the evaluation's observation period, which lasted from random assignment through calendar year 1996. On average the observation period was three and a half years. These gains have been discounted to reflect their value in 1994, the base year for this analysis. (Project NetWork's estimated effects on earnings after 1996 are discussed below). As the exhibit shows, the earnings gain during the observation period was $509 per treatment group member for the full sample.[2]

The compensation of individuals in the treatment and control groups also included fringe benefits, notably legally-required benefits, employer-paid health and life insurance, pension contributions, and workers' compensation. Based on national data,[3] these benefits were estimated to be 15 percent of earnings. As a result, the net present value of increased fringe benefits was estimated to be $69 per treatment group member, lifting the overall compensation gain for the full sample to $585.

6.3.2 Tax Payments

Because of Project NetWork's impact on earnings, there was a small increase in the taxes paid by the eligible population, estimated as the change in federal and state income taxes, Social Security taxes, and state sales and excise taxes paid by members of the treatment group. Federal and state tax rules in effect in 1994 – including rules for tax credits such as the federal Earned Income Tax Credit – have been applied to the earnings and (where

[2] This estimate differs from the figure one would calculate by adding together the annual earnings impacts reported in Exhibit 4.1,because those estimates excluded 1993 impacts and did not discount impacts in years after 1994.

[3] U.S. Bureau of the Census (1995). Most of the fringe benefits (8 percent) were legally required (e.g., FICA, Unemployment Insurance, and workers compensation). Nonmandatory benefits, including insurance, retirement, and other benefits, which were received by only a subset of workers, averaged 7 percent across all workers.

Exhibit 6.4
Effects on Earnings, Fringe Benefits, and Tax Payments per Treatment Group Member for the Observation Period (in 1994 dollars)

Outcome	
Earnings and Fringe Benefits	
Earnings	$509
Fringe Benefits	76
Total Compensation	585
Tax Payments	
Federal Income Tax	-$31
State Income Tax	4
Social Security Tax	39
Sales and Excise Tax	6
Total Taxes	**18**

appropriate) SSI and SSDI payments of individuals in the treatment and control groups during the observation period. (See Appendix E for further explanation). The resulting estimates of Project NetWork's effects on these taxes are presented in Exhibit 6.4.

The largest tax increase was in Social Security payroll taxes, which increased by an estimated $39 per treatment group member in the full sample. This effect simply reflects the program's impact on earnings. There was a net reduction in federal income taxes because Project NetWork's earnings gain led to an average increase in the earned income tax credit (EITC) of $60 per treatment group member, which was larger than the $29 increase in income taxes. The estimated effects of the program on state income, sales and excise taxes were small, because increases in these taxes due to earnings gains were partly offset by reductions due to SSI and SSDI payment reductions.

6.3.3 Transfer Payments and Administrative Costs

As indicated earlier in this report, the measured treatment-control differences in SSI and SSDI payments indicate a small, but steady, stream of savings in these payments during the time covered by the impact analysis. The cost-effectiveness analysis has estimated the demonstration's effects on these two transfer payments over this observation period, presented in Exhibit 6.5, and in future years (see below). Again, all effects have been discounted to reflect their value in 1994.

Project NetWork's effects on other transfer program payments and administrative costs were estimated using survey data. Estimates of the program's effects on Food Stamps, Medicaid, and Unemployment Compensation from random assignment through the survey administration date – on average about two and a half years – are presented in Exhibit 6.5. The estimated effects on both payments and administrative expenses are small for all three of these programs.

Exhibit 6.5
Effects on Transfer Payments and Administrative Costs per Treatment Group Member for the Observation Period (in 1994 dollars)

Type of Payment or Cost	
Transfer payments	
SSI	-$42
SSDI	-149
Food Stamps	+16
Medicaid	+7
UI compensation	-1
Total	-169
Administrative costs	
SSI	-$1
SSDI	-3
Food Stamps	+1
Medicaid	+1
UI Compensation	0
Total	-2

SOURCE: Calculations from SSI and SSDI payment records, survey data, and Social Security Administration data on payments and administrative costs. The observation period lasted 30 months for SSI payments and 42 months for SSDI payments. Other payments, which cover 30 months, were estimated based on payments received in the month prior to the survey.

Exhibit 6.6
Effects on Benefit Components, per Treatment Group Member, during the Observation Period, and Extrapolation Period (in 1994 dollars)

Outcome	Observation Period	Extrapolation Period	Five-year Follow-up
Earnings and fringe benefits	$585	$240	$825
Taxes	18	11	29
SSI and SSDI payments	-191	52	243
Other Transfer Payments	23	42	65
SSI and SSDI administration	-4	-1	-4
Other transfer administration	2	4	6

6.3.4 Effects Beyond the Follow-up Period

The results presented thus far consider only program effects during the observation period. Some of these effects may have persisted beyond this period. In particular, relatively long-term impacts on earnings have been observed in a number of studies of training programs.[4] In the present case, it is not clear whether the earnings gains produced by the demonstration lasted beyond the observation period. On one hand, the estimated impact on earnings in the third calendar year after random assignment was essentially zero (-$23) for the portion of the sample for which three years of follow-up data are available. On the other hand, the 28 percent of the sample for whom only two years of follow-up data are available experienced much larger earnings gains in the first two years after random assignment than did the subgroup for whom three years of data are available. Earnings impacts may therefore have persisted longer for this subgroup.

Exhibit 6.6 shows the effects of projecting earnings gains for sample members with only two years of follow-up data and impacts on disability benefits for the entire sample beyond the observation period. We assumed that these impacts declined linearly to zero at the end of the fifth calendar year after random assignment.[5] We believe that these estimates provide reasonable upper bound estimates of the net benefits of the demonstration. In the remaining sections of this chapter, we discuss the effects of these projected benefits and costs on the conclusions of the cost-effectiveness analysis.

6.4 Results for the Full Sample

Exhibits 6.7, 6.8, and 6.9 summarize the cost-effectiveness results for Project NetWork from the perspectives of disabled persons, the Social Security Administration and the rest of the federal government, state and local government, and society as a whole. The exhibits provide two sets of estimates of the benefits and costs of Project NetWork. One set of results includes the projected program effects presented in Exhibit 6.6. The other is limited to the effects that were actually observed. All results are expressed in 1994 dollars.

6.4.1 Results for Persons with Disabilities

As indicated in Exhibit 6.7, the demonstration produced modest economic gains for persons

[4] See Bell, *et al*. (1995), GAO (1996), Couch (1992), Bloom (1984), Decker and Thornton (1994), Ashenfelter (1978), Kemper, Long, Thornton (1983), and Friedlander and Burtless (1995).

[5] The estimated impact on annual earnings in year two for the subgroup with only two years of follow-up data was $544. The projected impacts in years three, four, and five for this subgroup are $389, $233, and $78.

with disabilities. From this perspective, the gains in earnings and fringe benefits caused by Project NetWork are treated as gains and the savings in SSI and SSDI payments are counted as costs. The other pertinent outcomes are taxes, Food Stamps, Medicaid, and Unemployment Compensation. Overall, the estimated gain in earnings and fringe benefits more than offset losses in SSI, SSDI, and taxes, producing a net present value of $399 per treatment group member during the observation period. The conclusion that the program is cost-effective for people with disabilities is not dependent on assumptions regarding future program effects, although the estimated net present value is larger ($618) when projected effects are taken into account.

6.4.2 Results for Taxpayers

Project NetWork generated a net loss for taxpayers. Exhibit 6.8a (last column) shows that, for all levels of government combined, the savings in SSI and SSDI payments during the observation period, together with reduced service costs in other programs and increases in tax payments, are not enough to offset the net cost of the demonstration program. The same conclusion holds for the Social Security Administration (first column of Exhibit 6.8a) and other Federal agencies (second column), taken by themselves. State and local governments, however, enjoyed savings due to the displacement of VR services by demonstration services.

Once again, the conclusion for this perspective – in this case that the program is not cost-effective for taxpayers – is not dependent on assumptions regarding future program effects. Inclusion of projected effects (Exhibit 6.8b) leaves the net present value of the demonstration decidedly negative for all levels of government except state and local.

Exhibit 6.7
Benefits And Costs To Disabled Persons

Component	Observation Period	Total (Including Projection Period)
SSI and SSDI payments	-$191	-$243
Other transfer payments	23	65
Earnings and fringe benefits	585	825
Tax payments	-18	-29
Net Present Value	**$399**	**$618**

SOURCE: See Exhibits 6.2 and 6.3.

NOTES: Results include estimates of program effects extrapolated beyond the observation period (see Exhibit 6.6) and are expressed in 1994 dollars. Rounding may cause slight discrepancies in calculated sums.

Exhibit 6.8a
Benefits And Costs To Government in the Observation Period

Component	Social Security Administration	Other Federal Government	State and Local Government	All Levels of Government
SSI and SSDI payments	$185	$0	$6	$191
Other transfer payments	$0	-16	-8	-23
Transfer Program Administration	4	-1	-1	2
Tax payments	39	-31	10	18
Project NetWork Cost	-$3,660	$0	$0	-$3,660
Non-NetWork Costs	$0	$0	$1,453	$1,453
Net Present Value	-3,432	-$48	$1,460	-$2,019

SOURCE: See Exhibits 6.2 and 6.3.

NOTES: Results include estimates of program effects extrapolated beyond the observation period (see Exhibit 6.6) and are expressed in 1994 dollars. Rounding may cause slight discrepancies in calculated sums.

Exhibit 6.8b
Benefits And Costs To Government, Including Projections

Component	Social Security Administration	Other Federal Government	State and Local Government	All Levels of Government
SSI and SSDI payments	$223	$0	$10	$243
Other transfer payments	$0	-44	-21	-65
Transfer Program Administration	4	-3	-3	-2
Tax payments	55	-40	14	29
Project NetWork Cost	-3,660	$0	0	-3,660
Non-NetWork Costs	0	0	1,453	1,453
Net Present Value	-$3,368	-$87	$1,453	-$2,002

SOURCE: See Exhibits 6.2 and 6.3.

NOTES: Results include estimates of program effects extrapolated beyond the observation period (see Exhibit 6.6) and are expressed in 1994 dollars. Rounding may cause slight discrepancies in calculated sums.

6.4.3 Results for Society

Finally, our cost-effectiveness conclusions from the social perspective are shown in Exhibit 6.9. The program's estimated net present value is negative, although the net loss is somewhat smaller if projected benefits are included. This indicates that the results are not sensitive to assumptions about future benefits of Project NetWork. Overall, then, Project NetWork resulted in a transfer of resources from taxpayers to disabled persons and from the Federal government to state and local governments. For each dollar of net benefits to disabled

persons, taxpayers paid $3.24 to $5.06.[6] Thus, only if taxpayers place a large premium on seeing disabled individuals become more self-sufficient can the program be justified on cost-effectiveness grounds.

Exhibit 6.9
Benefits And Costs To Society

Component	Observation Period	Total (Including Projection Period)
Earnings and fringe benefits	$585	$825
Transfer Program Administration	2	-2
NetWork Cost	-3,660	-3,660
Non-NetWork Costs	1,453	1,453
Net Present Value	-$1,620	-$1,384

SOURCE: See Exhibits 6.2 and 6.3.

NOTES: Results include estimates of program effects extrapolated beyond the observation period (see Exhibit 6.6) and are expressed in 1994 dollars. Rounding may cause slight discrepancies in calculated sums.

[6] These figures are the ratios of the net cost to taxpayers (all levels of government) to the net benefit to disabled persons, including and excluding projected impacts, respectively.

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Appendix A
Data Sources for the Estimation of Demonstration Impacts

The three key sources of data used in the analysis of demonstration impacts are: SSA administrative data from the MBR, SSR, and MEF files; random assignment log and Case Management Control System(CMCS) data; and, in-person interviews with treatment and control group members conducted at baseline and follow-up. Each source is described below.

A.1 SSA Administrative Data

SSA administrative records provide complete records of SSI and SSDI benefit receipt, annual (calendar year) earnings records[1], and baseline demographic variables. Here, we briefly summarize how we obtained these records and the contents of the analysis files.

A.1.1 SSI and SSDI benefit files

SSA administrative records are the most reliable source of information on two key outcomes of interest: the receipt of SSDI and SSI benefits. These data, taken from the MBR810/811 file for DI beneficiaries, and the SSR831 file for SSI recipients, are available on a monthly basis, providing an essentially complete benefit history during the predemonstration period, as well as for the postdemonstration period up to the time of data extraction.

Two benefit history analysis files were created, one summarizing monthly SSI benefit receipt and the other monthly receipt of SSDI, for all individuals solicited to participate in Project NetWork. These files were created through a collaborative process between SSA/ORES staff, Abt Associates, and our computer programming vendor, Fu Associates Ltd. The file creation process is documented in two reports prepared by Fu Associates (1998a, 1998b).

The SSI benefit file contains monthly data from January 1990 through December 1996 on the payment status, the state supplementation code, the amount of earned and unearned income, the dollar value of the SSI federal assistance amount, and the dollar value of the SSI state supplementation. In addition, the file contains information on the total number of months of eligibility for SSI benefits prior to January 1990.

[1] To protect the confidentiality of the annual earnings records, all analysis of these records was conducted on-site at SSA/ORES offices by SSA staff.

The SSDI file contains monthly benefit information from January 1990 through December 1997, including benefit status codes and the dollar value of monthly benefit. The SSDI analysis file also contains the date of first month of eligibility for SSDI benefits, the total number of months of payment eligibility prior to January 1990, and the date of conversion to the SSA Old Age program.

A.1.2 Annual earnings data

SSA administrative records also provide information on SSA-covered earnings. The Master Earnings File (MEF), which contains annual (calendar year) SSA-covered earnings reported by employers, is the source of these data. To protect confidentiality, all analysis of these data was performed on-site at SSA offices by SSA/ORES staff. SSA/ORES staff provided us with means and variances of earnings for the treatment group and control group for the overall sample and for each of the subgroups analyzed.

A.1.3 Baseline demographic information

SSA administrative records (the MBR831, MBR810/811, and SSR831 source files) were also used in the derivation of the universe of eligible individuals solicited for the demonstration and to collect information on primary impairment and basic demographic information.[2] The procedures used to prepare the analysis file containing these data are documented in a report produced by Abt Associates (1998). The demographic variables collected from SSA administrative data include gender, race, age, permanent disability code, and primary impairment. The last three were measured at the time of random assignment. Appendix B discusses how we used the administrative files to calculate these baseline variables.

A.2 Random Assignment Log and CMCS file

The random assignment log maintained by Abt Associates was used to track the personal identification numbers, date of random assignment, and random assignment status of the 8,248 persons who volunteered for the demonstration. The Case Management Control System (CMCS) is a management information system used in the demonstration sites to record demographic information about demonstration volunteers and participation in case/referral management activities for those assigned to the treatment group.

[2] Unfortunately, electronic records generated for the mail solicitation of existing SSDI and SSI beneficiaries were lost during the early phase of the demonstration. In addition, no electronic records were maintained to document which new SSI applicants were solicited for Project NetWork. As a result, the data base development effort used to create the analysis file of solicited individuals relied on the simulated recreation of the universe of Project NetWork eligibles, using information about the schedule for mail solicitation in each demonstration site, the timing of solicitation of new SSI applicants, and administrative data on the receipt of SSI and SSDI benefits. Specifically, the analysis sample was constructed by including individuals who, according to administrative records, applied for SSI during the sample intake period or were receiving SSI or SSDI benefits in the month prior to the scheduled mail solicitation.

The CMCS is also a source of information on some demographic variables, although the demographic information on this file is generally regarded as less reliable than similar information in the SSA administrative files discussed above. We use the demographic information in the CMCS to measure years of education at random assignment and marital status at random assignment. We also use the CMCS to impute a small number of baseline demographic variables which were missing from the SSA administrative files discussed above. Appendix B discusses these variables in greater detail.

A.3 In-Person Interviews

In-person interviews are the third source of data for the evaluation of demonstration impacts. We conducted baseline interviews with a sample of treatment, control, and nonparticipant cases in all demonstration sites over the period March 1993 through December 1994. Interviews were conducted after random assignment for participants, and after solicitation for nonparticipants. Information collected from interviews with nonparticipants was used to analyze participation in Project NetWork (see Burstein *et al.* (1999)). A total of 3,439 baseline interviews were completed, including 2,555 with treatment and control group members, and 884 with nonparticipants. Response rates were 87 percent for participants, 53 percent for existing beneficiaries and recipients sampled as nonparticipants, and 49 percent for new SSI applicants sampled as nonparticipants.

Treatment and control group members were sampled from the random assignment file maintained by Abt Associates. The baseline survey used a stratified design to balance the sample across the eight demonstration sites and to oversample SSI applicants and recipients age 18-30. Nonparticipant sampling began in August 1993 for SSDI beneficiaries and SSI recipients solicited to participate in the demonstration in June of that year. Sampling for this and subsequent outreach cohorts was timed to occur approximately three months after the invitation to participate.[3] Nonparticipant sampling for new SSI applicants occurred all at once in May 1994, the first time sampling data became available, and included cohorts who applied for benefits in late 1993 and early 1994.

Follow-up interviews were conducted with participants from June through November 1996. Interviews were attempted with all treatment and control group cases who completed a baseline interview and who were randomly assigned on or after June 1, 1993. Restricting the sample to those who were randomly assigned later in the demonstration was done to ensure that the maximum length of time between random assignment and the interview attempt for any respondent would be 36 months. Altogether, 1,521 follow-up interviews were completed, for

[3] An 80-day waiting period gave potential participants time to volunteer prior to sampling. Data on an earlier outreach cohort indicated that 80 percent of eventual participants volunteered within 80 days.

a final response rate of 83 percent.[4] Across the two waves of interviews, then, the combined response rates for participants is 72 percent.[5]

A.3.1 Baseline and Follow-up Questionnaires

The baseline survey instrument was designed under a contract with Lewin-ICF, Inc (now The Lewin Group), and contains questions about education and training, health and functional limitations, transportation limitations, employment history, personal attitudes and outlook, knowledge of SSA=s work incentives for disability beneficiaries, and income and benefits. The survey also contains a wide array of questions about emotional stability, drug/alcohol use, and cognitive functioning.

The follow-up survey questionnaire was designed by Abt Associates and contains questions on health and functional limitations, education, receipt of training and rehabilitation services, transportation and child care, employment and earnings, personal attitudes and outlook, and income and benefits from a range of sources. The follow-up survey also provides respondent assessments of Project NetWork and measures of the extent to which participants understood the rules determining SSI and SSDI benefit levels and eligibility, and what effect the demonstration waivers had on these rules.

A.3.2 Interview Procedures

All interviews were in-person and most occurred in the respondent=s home. Interviewers administered electronic survey questionnaires using lap-top computers and computer-assisted personal interviewing (CAPI) techniques. Most questions required a simple limited-choice answer (e.g., yes/no, or "choose one of the following"); only in a few instances were respondents required to provide short-answer responses (e.g., type of occupation). The questionnaire incorporated allowable response categories and skip patterns directly into the CAPI software. Each respondent received $20 upon completion of the interview.

SSA was very sensitive to the physical limitations of the population being surveyed. Flash cards listing response categories were used in all interviews. Signers facilitated the interview process for those with hearing impairments, braille flashcards were used for those with sight impairments, and a Spanish version of the instrument was developed for respondents speaking Spanish as a first language (interpreters were provided as needed for respondents who spoke neither English nor

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[4] The original sample of 1,913 follow-up sample members included 77 individuals w ho moved to a location ourside of their original state of residence, where the baseline interview had been conducted. As agreed upon with the Co-Project Officers, and after a thorough analysis of the characteristics of these individuals and the implications of attempting to complete in-person interviews with them, these 77 “movers” were excluded from the original sample. The final response rate is therefore calculated as 1,521/1,836=82.8 percent.

[5] For participants, the baseline survey sample included 2,930 cases, with 2,555 completed interviews, for a response rate of 87 percent. For the follow-up survey, interviews were attempted with participants who completed a baseline interview and who were randomly assignment on or after June 1, 1993. The total follow-up sample was 1,836, and 1,521 interviews were completed for a response rate of 83 percent. Across the two waves, the combined response rate is therefore .87*.83=.72.

Spanish). In addition, the respondent could make use of a proxy (e.g., family member or friend) to assist with some or all of their responses.

Two survey field managers tracked the status of interviews from issuance to completion, maintaining regular telephone and electronic mail communication with individual interviewers and central office survey management staff. Ten percent of the respondents with telephones were recontacted to verify that interviews took place and that the $20 incentive payment was received.

Completed questionnaires were transmitted electronically to Abt=s central office survey management staff. Survey management staff monitored the completion of interviews across demonstration sites and across samples and prepared the baseline and follow-up survey data files. In preparing the data files, survey data were examined and cleaned to ensure that responses were within allowable ranges. In addition, verbatim responses pertaining to medical condition and employment industry and occupation were examined and assigned codes. Medical conditions were coded by experienced medical coders using the International Classification of Diseases, 9th Revision, Clinical Modification (I-9) (ICD-9 codes)[6]. The U. S. Department of Commerce index of industries and occupations[7] was used to code industry and occupation responses. The final survey data files contain one record per completed interview, uniquely identified by social security number. This unique identifier is used to link the baseline and follow-up survey data files.

[6] Craig D. Puckett, The Educational Annotation of ICD-9-CM, 4th Edition, Volumes 1,2,3 were used as a reference for this coding.

[7] U.S. Department of Commerce, Economics and Statistics Administration, Bureau of the Census, Classified Index of Industries and Occupations, 1990 Census of Population and Housing, 1990 CPH-R-4, April 1992.

Appendix B
Framework for Estimating Demonstration Impacts

The Project NetWork demonstration used a random assignment design to estimate program impacts. Volunteers for Project NetWork were randomly assigned to a group receiving case and referral management services from Project NetWork — the treatment group—and to a group not receiving the services from Project NetWork — the control group. Under random assignment, the groups should be statistically equivalent in all respects except for receipt of services from Project NetWork. Accordingly, any systematic difference in the subsequent outcomes of the two groups can be confidently attributed to the demonstration intervention. Throughout this report, the impacts of Project NetWork have been estimated as the difference in average outcomes between treatment and control groups.

In this appendix, we describe our methods for estimating the impacts of Project NetWork in greater detail. Throughout this discussion, we refer to original data sources which are described in Appendix A. The sections of this appendix describe:

- how we obtained the research sample of 8,248 individuals;
- how we used the data sources to create all variables used in the evaluation;
- tests of the reliability of the demonstration—tests of differences in baseline characteristics between treatment and control group members, and tests of selection bias in our follow-up survey of a subset of randomly assigned sample members;
- our methodology for estimating program impacts on outcomes based on administrative data on SSI and SSDI benefits for the full sample, and impacts on outcomes based on follow-up survey on a subset of sample members; and,
- our methodology for estimating program impacts on earnings based on annual earnings data from administrative records.

The appendix also includes a supplementary exhibit (Exhibit B.5) showing estimated impacts on reported service receipt, by impairment group.

B.1 Defining the Research Sample of Randomly Assigned Persons

The research sample for the impact study consists of a total of 8,248 persons who volunteered to participate in Project NetWork. The personal identification numbers of these persons, and the month in which each was randomly assigned, were recorded in the random assignment log file maintained by Abt Associates. Of these persons, 4,160 were randomly assigned to the treatment group and 4,088 to the control group. SSA established enrollment targets for the demonstration in each site, and these were met in all sites except one. The primary recruitment source was a sequence of quarterly mailings from SSA to random subsets of existing SSDI beneficiaries and

SSI applicants/ recipients. These mailings accounted for 60 percent of all volunteers. The second largest source of participants was the solicitation of new SSI applicants during the SSI application process, which accounted for 21 percent of all volunteers. Additional recruitment sources included self-referrals (7 percent), other agency referrals (6 percent), and other sources (7 percent).[1]

We measure outcomes over the follow-up period in terms of months from the month of random assignment. We used the month of random assignment from the random assignment log file for this purpose.[2] The exception to this rule is the set of outcomes measured by the follow-up survey at the time of the interview. These measures include service receipt and measures of health and well-being.

We analyzed impacts on all 8,248 research sample members, even those who never received SSI or SSDI benefits. Because of the nature of solicitation and random assignment, 1,870 of the 8,248 research sample members did not receive an SSI or SSDI benefit at random assignment. The vast majority of these 1,870 persons never received SSI or SSDI benefits over the course of the entire follow-up period. Of these 1,870 persons, 1,413 were either denied SSI applicants or SSI applicants whose applications were still pending at the month of random assignment. The remaining 457 persons were referred to Project NetWork from other agencies.

B.2 Creation of Baseline Variables

This section describes how we used the original data sources to construct all baseline variables used in the impact study. By "baseline variables," we mean variables defined at or before the time of random assignment. These variables are used to define subgroups for impact analysis, and as independent variables in regressions used to estimate program impacts. These independent variables improve the precision of our impact estimates by controlling for chance differences in baseline variables between the treatment and control groups.

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[1] The analysis of demonstration participation, including participation rates by key subgroups of the eligible population is provided in Burstein *et a*l (1998).

[2] Electronic records generated for the mail solicitation of existing SSDI and SSI beneficiaries during the early phase of the demonstration were not retained. In addition, no electronic records were maintained to document which new SSI applicants were solicited for Project NetWork. As a result, the data base development effort used to create the analysis file of solicited individuals relied on the simulated and probably imperfect recreation of the universe of Project NetWork eligibles, using information about the schedule for mail solicitation in each demonstration site, the timing of application to SSI, and administrative data on the receipt of SSI and SSDI benefits. Specifically, the analysis sample was constructed by including individuals who, according to administrative records, either applied for SSI during the sample intake period or were receiving SSI or SSDI benefits in the month prior to the assumed scheduled mail solicitation.

B.2.1 Baseline Variables Defined for the full sample, and used in the analysis of impacts on SSI and SSDI benefits

The following independent variables used in the regression analysis of impacts on SSI and SSDI benefit receipt, along with outcome measures of benefit receipt based on administrative data, are defined for all 8,248 randomly assigned persons:

- Primary impairment (dummy variables for mental, neurological, musculoskeletal, missing, other impairment)
- Permanent disability code (dummy variables for permanently disabled, not permanently disabled, and code missing)
- Gender (dummy variables for female, male, missing)
- Race (dummy variables for white, African American, other, missing).
- Age (dummy variables for 18-30, 31-45, 45-59, 60 and above)
- Education (dummy variables for dropout, high school graduate, some college but not 4 years, 4 years of college or more, missing)
- Marital status (dummy variables for married female, married male, never married, divorced/widowed/separated, missing)
- Demonstration site (8 dummy variables)
- Dummy variable indicating whether person received SSI at random assignment
- Dummy variable indicating whether person received SSDI at random assignment
- Value of SSI benefit at random assignment (zero if no benefit received).
- Value of SSDI benefit at random assignment (zero if no benefit received).
- Number of months of SSI receipt prior to random assignment (dummy variables for none, 1-12 months, 13-36 months, 37-60 months, more than 60 months)
- Number of months of SSDI receipt prior to random assignment (dummy variables for none, 1-12 months, 13-36 months, 37-60 months, more than 60 months)
- Estimated interval between month of solicitation and month of random assignment (dummy variables for random assignment more than 1 month before solicitation, random assignment more than 6 months after solicitation, other).

Baseline demographic and impairment variables created using SSA administrative records

We used SSA administrative records to create a basic set of baseline demographic and impairment variables defined comparably for all members of the research sample.

General approach. The SSA administrative files used to obtain the demographic and impairment variables were the MBR 810/811, MBR831, and SSR831. These files contained one record per person as identified by a Personal Account Number (PAN). The record layout for the SSR831 included data variables for up to four applications for each PAN. Each application included

information unique to that application for SSI benefits. The record layout for the MBR831 included two sections per record. Data are present in the first section of the MBR831 record if the beneficiary received SSDI benefits under one type of entitlement and both sections if the beneficiary received SSDI benefits under dual entitlement. For this task we extracted data from the MBR 810/811, all four application groups on the SSR831, and both sections from the MBR831.

Within each application group on the SSR and each section on the MBR831, multiple occurrences may exist for these variables, corresponding to entitled claims, denied claims, and appeals. We devised a process for identifying the value of each potentially time-varying variable (such as impairment) closest to the random assignment date, and a process for selecting values of unchanging variables (such as gender) when multiple observations were in conflict.

The general approach for identifying the value for the analysis file was to:

1. Identify the "best"[3] value on the SSR or classify as missing
2. Identify the "best" value from the MBR, or classify as missing
3 Compare the values on the MBR and SSR, and select the value closest in time and preceding the month of random assignment.

Primary Impairment: Primary impairment information is captured in up to two separate application groups on the MBR831 file and on up to four application groups in the SSR831. Within each application group on the MBR831 file, up to five occurrences of primary impairment are possible for each individual, corresponding to MBR data, the Last Initial application (AL=A) , the Last Noninitial application (AL not=A), Additional MBR Adjudicative Data No. 1, and Additional MBR Adjudicative Data No. 2. In the SSR831, up to three occurrences of each primary impairment variable are possible, corresponding to SSI data, Last initial application (AL=A) and Last Noninitial application (AL not=A).

We confirmed our expectation that the impairment values contained in the MBR and SSR files are four-digit numeric codes. SSA/ORES staff provided a data dictionary and coding scheme that was used to classify the impairment codes into 22 categories. All of the values fell within the allowable range specified in the data dictionary.

The steps to identify primary impairment were as follows. First, all impairment codes (each occurrence of primary impairment) were recoded to the codes provided in the documentation from SSA. Values in the "ill-defined" category were recoded to the missing category. This resulted in a total of 21 values for each impairment variable.

[3] "Best" refers to either the one unambiguous value on the file, or, if more than one value occurs, the value corresponding to the application closest in time and preceding solicitation for Project NetWork.

One impairment value was selected for each application group (4 on the SSR, 2 on the MBR) in the SSR and MBR files. The order of priority for selection from among the multiple occurrences in each application was provided to us by staff in SSA ORES, based on the rationale of selecting the value believed to be most reliable .

The steps in the selection process were the following:

1) Go to the DIG in the MBR or DIB DIG in the SSR; if present take this value.
2) If the impairment code is still missing, go the Last Noninitial data (representing appeals); if present take this value
3) If the impairment code is still missing, go to the Last Initial data; if present take this value
4) If the impairment code is still missing, go to the additional adjudicative data (MBR only); if present take this value.

If primary impairment was missing in all application groups, the secondary impairment value was used, and the same process as above was followed. Secondary impairment was used in well under 1 percent of cases in both the SSR and the MBR.

The next step was to resolve the cases in the MBR and SSR for which it was impossible to identify one unique value for impairment. Well under 10 percent of research sample members had this type of data conflict. To do so, Abt staff determined which application was closest in time and preceding random assignment and selected the impairment code from that application. This was achieved by comparing date of filing (DOF) on the MBR or application date (APPL) in the SSR to the random assignment month. If no application preceded random assignment (approximately 10 percent of persons with more than one possible primary impairment, or less than 1 percent of the research sample) Abt staff selected the application closest in time and after random assignment.

The result of these steps was one unique impairment value for the SSR file and one unique value for the MBR file. Abt staff then merged the two files to determine the extent to which the primary impairment values on the MBR agreed with the SSR information. About 10 percent of research sample members had conflicting impairment values from these two files. To identify the impairment value for these cases, Abt staff selected the value with the application date closest in time and preceding random assignment.

Values for this variable are:

0	Missing	5	Schizophrenia
1	Infectious and parasitic	6	Psychoses and neuroses
2	Neoplasms	7	Mental Retardation
3	Endocrine and metabolic	8	Central Nervous System
4	Blood and blood forming organs	9	Diseases of the Eye

10	Diseases of the Ear	15	Complications of pregnancy
11	Diseases of the Circulatory System	16	Skin and subcutaneous
12	Diseases of the respiratory system	17	Musculoskeletal system
13	Diseases of the digestive system	18	Congenital anomalies
14	Diseases of the genitourinary system	19	Perinatal disease
		20	Injury

In the impact study, we analyzed four major subgroups defined by primary impairment. The four groups are mental (categories 5-7 above), neurological (category 8), musculoskeletal (category 17) and other (all other categories). The rate of data conflicts for these much larger aggregations of primary impairment types is much smaller than the already low rate of data discrepancies for the 21 primary impairment types.

The distribution of all impairment categories among treatment and control group members is shown in Exhibit B.1.

Exhibit B.1
Characteristics of the Project NetWork Treatment and Control Groups: Primary Impairment

Characteristic	Control Group	Treatment Group
Sample Size	*4,088*	*4,160*
Primary Impairment		
Infectious and Parasitic	3%	3%
Neoplasms	1	2
Endocrine and Metabolic	5	4
Blood and blood forming organs	0	1
Schizophrenia	12	11
Psychoses and Neuroses	23	24
Mental Retardation	8	8
Central Nervous System	5	6
Diseases of the Eye	3	2
Diseases of the Ear	1	1
Diseases of the circulatory system	7	6
Diseases of the respiratory system	2	2
Diseases of the digestive system	1	1
Diseases of the genitourinary system	2	2
Complications of Pregnancy	0	0
Skin and subcutaneous	0	0
Musculoskeletal System	12	13
Congenital anomalies	0	0
Perinatal disease	0	0
Injury	6	6
Missing	8	7

Source: SSA Administrative Records (SSR831, MBR831, MBR810/811 source files).

Permanent Disability Code: The source data also contained information regarding whether or not an individual's disability is considered permanent. This permanent disability code could have up to four occurrences per application in the MBR and up to two occurrences per application in the SSR. According to documentation provided by SSA, the values for this variable should be either N (not permanent) or P (permanent). An additional code, O for cessation, appears for some cases. The same process of selection used for impairment was used to identify the final value for this variable.

We defined three dummy variables reflecting this code -- one indicating permanent disability, one indicating not a permanent disability, and a third indicating that this code is missing.

Gender:. A slightly different process was used to identify gender. There is only one occurrence of gender in each application group. For less than 1 percent of the sample, there were conflicting values within the SSR and/or the MBR files. These conflicts were resolved by examining the number of occurrences of each value for gender and selecting the most frequently occurring value. If no value occurred more frequently than the other, the value for gender was set to missing.

Race. Race is available on the MBR810/811 (one value per PAN) and on the SSR831 (up to four values per PAN corresponding to the four application groups). For about seven percent of persons, conflicting values existed. These conflicts were resolved the same way conflicts in the gender variable were resolved. That is, Abt staff examined the number of occurrences of each value for race and selected the most frequently occurring value. If no value occurred more frequently than others, the value was set to missing.

Values are:
Missing
B- Black
W- White
O-Other

Age SSA/ORES staff used administrative records to provide the most reliable measure of date of birth to Abt analysis staff.

Baseline demographic variables created for the full sample of 8,248 persons, using CMCS records

The CMCS was used to provide two additional demographic variables measured at random assignment -- years of schooling and marital status. There was a high rate of missing data for years of education (missing for nearly half of the eligible population) in the administrative data and marital status was not available. The CMCS provides only one value of these variables per person. We also used CMCS data to impute observations of demographic and impairment data

which were missing in the SSA administrative files. This imputation is reasonable because values of other demographic variables reported by both CMCS and SSA files are generally highly correlated.

Variables measuring receipt of SSI and SSDI benefit at random assignment, and prior to random assignment

The SSI and SSDI benefit files provide this information. The SSDI benefit file provides participation status and benefit levels for each month from January 1990 through December 1997, while the SSI benefit file provides participation status and benefit levels for each month from January 1990 through only December 1996. These two files also provide the total number of months of receipt of SSI and SSDI prior to January, 1990. Using this information, we calculated several measures of benefit receipt at random assignment -- participation in SSI and SSDI, the value of SSI and SSDI benefits, and the total number of months of prior receipt of SSI and SSDI benefits.

We define "participating" in SSDI or SSI in a month as receiving nonzero benefits in that month. In the SSDI files, the monthly participation status variables indicate whether a person is classified as currently receiving pay, died while eligible for benefits, terminated, suspended, or converted to old age social security. In the SSI files, the monthly participation status variables are roughly similar, but many more values exist to indicate reasons for termination and suspension. In both files, having a participation status code indicating receipt of benefits in a month and actually having a nonzero benefit in the month are consistent over 99 percent of the time.

The SSI benefit files provide separate monthly values for the federal SSI benefit and the state SSI supplement. Persons in all sites except Dallas and Fort Worth receive at least some supplemental benefits. The value of these state supplements is well below the federal payment, and averages about $1 per month for all 8,248 persons in the research sample. We used the sum of these benefits as the appropriate measure of the baseline SSI benefit amount.

B.2.2 Additional Baseline Variables Defined Only for the Survey Sample

For the subgroup of randomly assigned persons who responded to both the baseline and follow-up surveys, additional baseline variables were obtained from the baseline survey. These baseline variables are used for the analysis of impacts on outcomes obtained from responses to the follow-up survey. Of the 8,248 randomly assigned persons, a total of 1,521 completed both baseline and follow-up surveys. All survey-based impact estimates presented in this report are based on this sample of 1,521 persons who completed both surveys. In this section we list these variables, along with the survey questions used to calculate the variables, and the number of missing responses present.

Imputations

To prevent any loss of observations, all missing values of baseline variables are imputed. As we indicate below, the number of missing responses is generally very small. The imputed values are based on means of non-missing observations calculated by subgroups based on treatment/control status and young SSI status.[4]

In the case of missing observations of several related dummy variables (for example, age dummies), a "die" is thrown to determine which one of the related dummy variables is given a value of 1. The chance of each dummy becoming "yes" is equal to the mean value of the non-missing observations of each dummy. For example, suppose the mean value of three age dummies is .20, .30, .50. In this case, the chance of receiving an imputed value of 1 is 20 percent for the first dummy variable, 30 percent for the second dummy variable, and 50 percent for the third dummy variable.

In the case of a missing observation of a single dummy variable (such as gender), a "die" is also thrown to determine whether the dummy is zero or one. The chance of this dummy receiving a value of 1 is equal to the mean value of the non-missing observations.

In the case of continuous variables, missing observations are imputed with the appropriate mean value (calculated by subgroups determined by treatment status and young SSI status) of non-missing observations.

Variables from the baseline survey

Native language: Dummy variable (1=yes, 0=no) indicates non-English speaker: (Question A6=2 or 3.) This variable has 11 missing observations

Living arrangement: Dummy variables (1=yes, 0=no) indicate residence is a private residence (Question A2=1), care home or group home (A2=2,3), nursing home (A2=4), or other residence (A2=5). This variable has 11 missing observations

Enrolled in an educational program: Dummy variable (1=yes, 0=no) based on question B4. This variable has 12 missing observations.

Ever attended school for those with disabilities: Dummy variable (1=yes, 0=no) based on question B6. This variable has 31 missing observations.

Received any form of training or rehabilitation within the last 12 months. This includes job training, job placement, vocational/business training, counseling/guidance, life/social skills, or

[4] The baseline and follow-up surveys over-sampled young SSI recipients.

other training/rehabilitation services: Dummy variable (1=yes, 0=no) based on question B8. This variable has 17 missing observations.

Has a physical/mental/other condition preventing respondent from working: Dummy variable (1=yes, 0=no) based on questions C19A and C16A. (Yes if question C19A=1, no if C19A=0 or C16A=0. This is set to missing if C19A is missing. There are 11 missing observations.

Number of days spent in bed during the previous 12 months because of respondent's disability. Dummy variables (1=yes, 0=no), based on question C23B, for zero days, less than one week, from one week to one month, more than one month. This is set to missing and imputed if C23B is missing. There are 65 missing observations.

Functional limitations: Respondent reported difficulty with any of the following activities: seeing newsprint; hearing normal conversation; having his/her speech understood; lifting and carrying; climbing a flight of stairs; walking a quarter-mile; using a phone: Dummy variable (1=yes, 0=no) which is yes if any of responses to questions C3A, C4A, C5A, C6A, C7A, C8A, or C9A is yes. This is set to missing if all of these responses are missing. There are 11 missing observations.

Limitations to daily living: Respondent reported difficulty with any of the following activities: getting around inside the home; getting around outside the home; getting in or out of bed or a chair; taking a shower/bath; dressing; eating; using the toilet; keeping track of money or bills; preparing meals; doing light housework. Dummy variable (1=yes, 0=no) which is set to yes if any of responses to questions CTA1, CTB1, CTC1, CTD1, CTE1, CTF1, CTG1, CTH1, CTI1, or CTJ1 is yes. This is set to missing if all of these responses are missing. There are 11 missing observations.

Household received AFDC, Food Stamps, or housing assistance in the month before random assignment. Dummy variables (1=yes, 0=no) based on questions G3A, G3B, G3C. There are, respectively, 35, 23, and 28 missing observations.

Other household members worked for pay in the previous year: Dummy variable (1=yes, 0=no) based on question G5. There are 36 missing observations.

Medical insurance coverage. The survey responses indicate whether the respondent is covered by Medicaid (Question G4A=1 and G4AY=1,3) Medicare Part A (G4B=1 and G4BY=1,3) Medicare Part B (G4C=1 and G4CY=1,3), Military care (G4D=1 and G4DY=1,3), employer-provided plan (E=1 and G4EY=1,3) or other private plan (G4F=1 and G4FY=1,3). Each of these is set to missing if the corresponding G4 question is missing. There are, respectively, 42,70,87,22,49, and 50 missing observations. The three dummy variables used in impact regressions indicate coverage under 1) either Medicaid, Medicare Part A or Medicare Part B, 2) either military care, employer plan, or a private plan 3) not covered.

Ever done any unpaid work. This includes work as part of a training program, at a center for persons with disabilities, for family, or elsewhere, within the past year. Dummy variable (1=yes, 0=no) set to yes if responses to any of questions BTAC, BTBC, BTCC, BTDC, or BTEC is yes. This is set to missing if all of these responses are missing. There are 14 missing observations.

Ever worked for pay in the previous 12 months. Dummy variable (1=yes, 0=no) set to yes respondent worked for pay last year (E3=1) or is currently working (E1=1) This is set to missing if E3 is missing. There are 14 missing observations.

Number of years worked before applying for disability benefits These dummy variables are based on responses to questions E2,E7A,E7B. They are set to zero if E2 indicates respondent never worked, or if E7A indicates respondent did not work before receiving benefits. They are set to missing if E7B is missing. There are 65 missing observations. The dummy variables used in impact regressions indicate the number of years worked is from 0 to 5, from 6 to 10, from 11 to 20, and more than 20.

Transportation limits respondents ability to work. Dummy variable (1=yes, 0=no) based on question D1. There are 25 missing observations.

Proxy responded to baseline interview questions. Dummy variable (1=yes, 0=no) based on question FS11.

Center for Epidemiological Studies (CES-D) depression screener. (Questions F5A-T)
In this screener that was designed to detect groups with significant depressive symptoms in epidemiological studies, responses to 20 questions are scaled from zero to three based on the frequency of the self-reported presence of the feelings described by the item.[5]

For these questions, respondents answered:
1. Rarely/none of the time
2. Some/little of the time
3. Occasionally
4. Most/all of the time

about each of these statements:

I was bothered by things that usually don't bother me.
I did not feel like eating.
I felt I could not shake off the blues.
I felt that I was just as good as other people.
I had trouble keeping my mind on what I was doing.

[5] Rupp *et al.* (1996)

I felt depressed.
I felt that everything I did was an effort.
I felt hopeful about the future.
I thought my life had been a failure.
I felt fearful.
My sleep was restless.
I was happy.
I talked less than usual.
I felt lonely.
People were unfriendly.
I enjoyed life.
I had crying spells.
I felt sad.
I felt that people disliked me.
I could not get going.

The CES-D scale is the sum of these responses, so higher numbers indicate a higher degree of depression. Missing/refused observations of each of the 20 responses were imputed with mean responses by treatment group/youngSSI status. About 10-15 observations were missing for each question. This scale has a mean of 22.9 and a standard deviation of 9.1.

Mental Health Inventory (MHI) Scale of responses to questions about feelings/emotions (Questions F9A-E). This test is a subset of the 38-item Mental Health Inventory used in the Health Insurance Experiment to measure mental health status.

For these questions, respondents answered:
1. All of the time
2. Most of the time
3. A good bit of the time
4. Some of the time
5. A little of the time
6. None of the time

about each of these questions:

A. Have you been a very nervous person?
B. Have you felt calm and peaceful?
C. Have you felt downhearted and blue?
D. Have you been a happy person?
E. Have you felt so down in the dumps that nothing could cheer you up?

The variable MHI (mental health inventory) scale is the sum of these responses. For this sum, the numbered responses to questions A, C, and E were reversed in this addition; in other words, the scale calculation adds 6 minus the response. So, for the MHI scale, higher numbers indicate a less positive state.

Missing/refused observations of each of the 5 responses were imputed with mean responses by treatment group/youngSSI status. About 10 observations were missing for each question. This scale has a mean of 14.2 and a standard deviation of 5.7.

Mini Mental State Examination (MMSE) (Questions F23-F33). The MMSE is used as a screener for cognitive impairment. Cognitive impairment refers to problems in the performance of such cognitive abilities as attention, memory, language, calculation, orientation, and reasoning. The MMSE was developed by Folstein, Folstein, and McHugh (1975).

Respondents are asked a series of objective, cognitive questions such as "what is the current year?" The MMSE scale is the sum of the number of correct responses. More points indicate a higher level of cognitive function. Whenever a respondent says that he/she cannot answer a group of questions because of a disability, the responses are coded as missing.

Missing/refused observations of each of the responses (including those missing because of disability) were imputed with mean responses by treatment group/young SSI status. About 10-35 observations were missing for each question, usually less than 10. This scale has a mean of 27.2 and a standard deviation of 2.3. 29% of respondents answered everything correctly. 90% received scores of 25 or higher.

Earnings in the 12 months prior to random assignment The calculation of these variables is based on the random assignment date, interview date, and responses to questions E1-E4. For those who are working now or have worked in the last 12 months, the survey asks for start and end dates of employment, pay and pay period (hourly, daily, weekly, every two weeks, monthly, yearly), hours per week, and days per week. We calculate earnings for the twelve months just before the random assignment date. The questionnaire does not ask about second jobs. Dates (and earnings) are coded as missing if the month and/or year is missing. If the day is missing but the month and year are present, the day is imputed as 15. If the respondent states (questions E1-E3) that he/she is not working now and hasn't worked in the last 12 month, earnings are assumed to be zero. If dates, amount of pay, pay period, hours per week, or days per week are missing, then baseline earnings and hourly wages were are all set to missing. About 150 observations of earnings are missing, the rest are positive or zero.

Follow-up survey month: This variable indicates the month of the follow-up survey, expressed as the number of months from the date of random assignment. The mean is about 33.

B.3 Creation of Outcome Variables

This section describes the creation of outcome variables used in the estimation of demonstration impacts, including measures of SSI and SSDI benefit receipt measured for the full research sample, and outcomes defined only for the follow-up survey sample.

B.3.1 Measures of SSI and SSDI Benefit Receipt

The SSI and SSDI benefit files provide this information. The SSDI benefit file provides participation status and benefit levels for each month from January 1990 through December 1997, while the SSI benefit file provides participation status and benefit levels for each month from January 1990 through only December 1996. These two files also provide the total number of months of receipt of SSI and SSDI prior to January, 1990. Using this information, we calculated several measures of benefit receipt over follow-up months since the month of random assignment. Because persons were randomly assigned in different months, a specific follow-up month (first, thirtieth, etc) corresponds to different calendar months for different persons.

We define "participating" in SSDI or SSI in a month as receiving nonzero benefits in that month, as we explained in the previous section discussing the creation of baseline variables. The SSI benefit files provide separate monthly values for the federal SSI benefit and the state SSI supplement. We used the sum of these benefits as the appropriate measure of the SSI benefit amount. Persons in all sites except Dallas and Fort Worth received at least some supplemental benefits. The value of these state supplements is well below the federal payment, and averages about $1 per month for all 8,248 persons in the research sample. All benefits are in terms of 1996 dollars.

B.3.2 Outcome Variables Defined Only for the Survey Sample

Earnings, total months worked, hours worked, hourly wages, employer-provided benefits. The calculation of these variables is based on the interview date, the random assignment date, and responses to questions E1-E3. For those who are working now or have worked since the random assignment dates, the survey asks for start and end dates of employment, pay and pay period (hourly, daily, weekly, every two weeks, monthly, every two weeks, twice a month, yearly, other), hours per week, and days per week. For those with more than one reported job, we sum earnings, months worked, and hours worked over all jobs, and we calculate hourly wages as the highest hourly wage received. If the respondent states (questions E1-E2) that he/she is not working now and hasn't worked in the follow-up period, earnings, hours, etc are assumed to be zero.

Dates (and earnings) are coded as missing if the month and/or year is missing. If the day is missing but the month and year are present, the date is imputed as 15. An observation of an outcome variable is missing if its calculation requires a missing date, amount of pay, pay period,

hours per week, or days per week variable. Observations of outcome variables that are missing either because survey data are missing for employed persons, or because persons are not employed, are currently all set to zero. Therefore, there are no missing observations for these outcomes. About 50 employed persons have one or more missing observations.

The survey respondents reported eligibility for employer-provided fringe benefits only for the most recent job spell.

Receipt of services since the month of random assignment. These dummy variables (1=yes, 0=no) are based on the responses to questions C7-C15A. The categories of services are physical therapy, psychological counseling, life skills/social skills, assessment of potential to work, occupational therapy, career guidance/job search assistance, training at trade/business school, college classes, job-related training/on-job-training, and other rehab/employment service. The respondents also indicated whether each of these services was received from Project NetWork or from a VR agency. Each of these is set to missing if the response is missing, refused, or don't know. There are, respectively, 14, 15, 12, 50, 16, 18, 11, 4, 9, and 10 missing observations. We did not impute these missing observations. Instead, we omitted observations with missing responses and estimated impacts using the remaining sample.

Variables measuring health and well-being

General health is very good or excellent: Dummy variable (1=yes, 0=no) based on response to question B1. This is set to missing if B1 is missing, refused or don't know. There are 11 missing observations.

Health has improved since random assignment: Dummy variable (1=yes, 0=no) based on response to question B2. This is set to missing if B2 is missing, refused or don't know. There are 15 missing observations.

Functional limitations: Respondent has trouble doing any of these-- seeing newsprint, hearing normal conversation, having his/her speech understood, lifting and carrying, climbing a flight of stairs, walking a quarter-mile, using a phone. This variable counts the number of "yes" responses to questions B4A, B5A, B6A, B7A, B8A, B9A, or B10A is yes. This is set to missing if all of these responses are missing. There are 11 missing observations.

Limitations to daily living: Respondent has trouble doing any of these -- getting around inside the home, getting around outside the home, getting in or out of bed or a chair, taking a shower/bath, dressing, eating, using the toilet, keeping track of money or bills, preparing meals, doing light housework. This variable counts the number of "yes" responses to questions B11A1, B11B1, B11C1, B11D1, B11E1, B11F1, B11G1, B11H1, B11I1, or B11J1 is yes. This is set to missing if all of these responses are missing. There are 11 missing observations.

Respondent has been an excessive drinker since random assignment. Dummy variable (1=yes, 0=no) based on response to question F19. This is set to missing if F19 is missing or don't know. There are 7 missing observations.

Respondent has used drugs to get high since random assignment Dummy variable (1=yes, 0=no) based on response to question F22. This is set to missing if F22 is missing or don't know. There are 10 missing observations.

Respondent stayed overnight in a hospital because of emotional problems. Dummy variable (1=yes, 0=no) based on response to question F28. This is set to missing if F28 is missing, refused or don't know. There are 6 missing observations.

Respondent has you felt depressed/sad for at least two weeks over the previous year Dummy variable (1=yes, 0=no) based on response to question F13. There are 145 missing observations.

Respondent feels better off than one year ago. Dummy variable (1=yes, 0=no) based on response to question F17. There are 168 missing observations.

Respondent feels he/she will be better off a year from now Dummy variable (1=yes, 0=no) based on response to question F18. There are 257 missing observations.

Illness/injury kept respondent from working for at least 7 days during the last year Dummy variable (1=yes, 0=no) based on response to question B23. Yes if B23_A (number of days) exceeds 6. There are 78 missing observations.

Respondent's condition prevents any work Dummy variable (1=yes, 0=no) based on response to question B16_A and B19_A. Yes if B19_A=1, no if B19_A=0 or B16_A=0. This is set to missing if B19_A is missing, refused, or don't know. There are 56 missing observations.

Respondent's condition prevents working full time (Questions B16_A,B20_A,B19_A) Dummy variable (1=yes, 0=no) which is no if B20_A=1 "able to work full time" or if B16_A indicates that respondent has no condition limiting his/her ability to work. There are 49 missing observations.

Transportation limits respondents ability to work. Dummy variable (1=yes, 0=no) based on question D1. There are 11 missing observations.

MHI Scale of responses to questions about feelings/emotions (Questions F16A-E) and the MMSE (Mini Mental State Examination) Scale of responses to basic cognitive questions (Questions F30-F40)

These two scales and the corresponding baseline scales are based on exactly the same sets of questions. The only difference is that imputations of missing responses in the follow-up survey are handled slightly differently. In the follow-up survey, responses to individual questions are still imputed (using means of non-missing variables, by treatment and young SSI status) when the respondent cannot answer a question because of a disability. However, once these imputations are made, additional imputations of a person's missing responses to individual questions are not made unless the total number of the person's remaining missing responses is 3 or less. Thus, 152 FMHI observations and 61 FMMSE observations are missing because these respondents had missing observations for 4 or more questions, and their disability was not the cause for these missing observations. The MHI scale has a mean of 14.4 and a standard deviation of 5.7. The MMSE scale has a mean of 27.1 and a standard deviation of 2.7.

B.4 Tests of the Reliability of the Demonstration and of the Follow-up Survey Data

In this section, we briefly summarize two sets of tests of the reliability of the impact estimates presented in this report. First, we compare baseline characteristics of treatment and control members to test whether the two groups are well-matched. Second, we compare these same baseline characteristics for the full sample of randomly assigned persons and the subgroup who responded to the follow-up survey. The results of these tests confirm that random assignment was successful and that the survey respondents are a representative subsample of the full sample of randomly assigned persons.

B.4.1 Comparison of Baseline Characteristics of the Treatment and Control Groups

Random assignment should ensure that the treatment and control groups were alike at the time of volunteering for the demonstration with respect to characteristics that were or were not measurable. Exhibit B.1 presents baseline characteristics of the treatment and control groups, obtained from administrative records data. As the exhibit demonstrates, the two groups were quite similar with respect to known, measured characteristics at the time of random assignment. The only significant differences observed are in the percentage of participants in the Minneapolis site (with a higher proportion of treatment group members) and in the Richmond site (with a higher proportion of controls) and in the percentage of blacks (with 27 percent in the control group and 26 percent in the treatment group this difference is significant at the 10 percent level). Since these are fewer differences than we would expect by chance alone, we conclude that random assignment was successful in producing well-matched treatment and control groups. Our impact estimates always control for any chance differences in observable baseline characteristics.

We conducted similar comparisons of subgroups whose impact estimates were analyzed in this report. These subgroups were formed on the basis of the four service models, four types of primary impairment at random assignment, and four titles of eligibility at random assignment. We

also conducted similar comparisons for survey respondents. These comparisons again indicated that random assignment produced treatment and control groups which were very similar at baseline, with differences in baseline characteristics no larger than one would have expected by chance.

Exhibit B.2
Characteristics of the Project NetWork Treatment and Control Groups

Characteristic	Control Group	Treatment Group
Sample Size	*4,088*	*4,160*
Demonstration Site		
Dallas	14%	14%
Fort Worth	9	9
Minneapolis**	11	13
Phoenix/Las Vegas	14	13
New Hampshire	13	13
Richmond*	14	13
Spokane/Coeur d'Alene	12	11
Tampa	13	14
Gender		
Female	42	42
Male	58	58
Age at Solicitation		
18-30	22	22
31-45	47	46
46-59	29	30
60 and over	2	3
Mean Age	40	40
Type of Disability Benefits received at Random Assignment		
SSDI only	38	38
SSI only	26	26
Concurrent SSDI/SSI	13	13
new SSI applicant	23	23
Average number of months received benefits prior to Random Assignment		
SSDI	36	36
SSI	28	28
Average Monthly benefits at Random Assignment for those receiving:		
SSDI	$608	611
SSI	$289	292
Primary Impairment		
Musculoskeletal	12	13
Neurological	5	6
Mental	42	42
Other	32	32
Race		
Black*	27	26
White	63	64
Other	4	4
Education		
<HS	22	21
HS Graduate	36	37
Some College	18	19
College Degree	8	8

* Difference is statistically significant at the 10 percent level

** Difference is statistically significant at the 5 percent level

***Difference is statistically significant at the 1 percent level

Source: SSA Administrative Records (SSR831, MBR831, MBR810/811 source files) and CMCS data.

B.4.2 Comparison of Baseline Characteristics of the Research Sample and Follow-up Survey Respondents

Several outcomes of interest, including employment, receipt of services, and measures of health and well-being, were based on responses to the follow-up survey conducted as part of the evaluation. For these measures to be generalizable to the overall demonstration research sample, the subsample of follow-up survey respondents should be a reasonably representative subgroup of the full sample of Project NetWork volunteers. Exhibit B.2 presents a comparison of baseline characteristics of all persons randomly assigned to the subgroup of follow-up survey respondents, again using data from administrative records. The means for the follow-up survey subsample are weighted to reflect the fact that our survey design oversampled young SSI recipients.

As this exhibit indicates, the follow-up survey respondents are in general very similar to the full sample of Project NetWork volunteers. Although some statistically significant differences exist, they are generally quite small. As indicated in the exhibit, the follow-up survey respondents were somewhat younger, more likely to have received SSDI at random assignment, and less likely to have been a new SSI applicant. In addition, the racial composition of the two groups differed, with a smaller percentage of blacks and a higher percentage of whites among follow-up survey respondents than for participants overall. In the impact estimates presented in this report, we control for differences in all measured baseline characteristics.

Exhibit B.3
Characteristics of all Persons Randomly Assigned Compared to Follow-up Survey Respondents

Characteristic	All Randomly Assigned	Follow-up Survey Respondents
Sample Size	*8,248*	*1,521*
Gender		
Female	42	44
Male	58	56
Age at Solicitation		
18-30***	22	28
31-45***	46	41
46-59	29	28
60 and over	3	2
Mean Age***	40	39
Type of Disability Benefits received at Random Assignment		
SSDI only***	38	41
SSI only	26	28
Concurrent SSDI/SSI	13	13
new SSI applicant***	23	18
Average number of months received benefits prior to Random Assignment		
SSDI	36	37
SSI	28	28
Average Monthly benefits at Random Assignment for those receiving:		
SSDI	$610	$610
SSI	$291	$294
Primary Impairment		
Musculoskeletal	12	12
Neurological	6	7
Mental	42	40
Other	32	41
Race		
Black*	26	23
White ***	64	68
Other	4	4
Education		
<HS	21	22
HS Graduate	37	38
Some College	19	18
College Degree	8	8

* Difference is statistically significant at the 10 percent level
** Difference is statistically significant at the 5 percent level
***Difference is statistically significant at the 1 percent level
Source: SSA Administrative Records (SSR831, MBR831, MBR810/811 source files).

B.5 Methodology for Estimating Impacts

In this section we describe the methodology used to estimate the net impacts of Project NetWork, including estimation of impacts on SSI and SSDI benefit receipt, outcomes measured in the follow-up survey, and grouped annual earnings.

B.5.1 Estimating Impacts on SSI and SSDI benefit receipt

The estimates of impacts of Project NetWork on SSI and SSDI benefit receipt use administrative data on these outcomes and are based on the full sample of 8,248 randomly assigned persons. The impact estimates are regression adjusted, using the baseline variables described in section B.2. The estimation procedure regresses the outcome variable on these independent variables and 8 dummy variables which are the treatment status indicator interacted with the site dummies. We also include a site-level heteroskedasticity correction.[6] The value of the coefficients of each of these dummies is the impact of PNW in each site.

The overall impact for the entire 8,248-person sample is a weighted average of the 8 site-level impact coefficients. The 8 impact coefficients are weighted so that each contributes equally (one-eighth) to the overall impact for the full sample. This weighting scheme is consistent with the notion that each site is a trial of Project NetWork services, so that the overall estimate reflects the results of 8 equally weighted trials. Similarly, the variance of this final estimate is the equally-weighted average of the variances of these 8 impact estimates. To estimate impacts for the entire sample served by each model, each of the two sites receives a weight of one-half. This procedure implicitly uses a weight for each person defined so that the sum of the weights for all persons in each site equals 0.125. In sites with less than one-eighth of the total sample of persons (less than 1,031 persons), each person receives a weight greater than one. In sites with more than 1,031 persons, each person receives a weight less than one.

In estimating impacts for subgroups defined by primary impairment or title of eligibility, we use only the sample of persons belonging to the subgroup. The same person-weights that were used in estimating impact for the overall sample are used to produce impact estimates for these subgroups. Because the proportion of persons who fall into these subgroups varies by site, using the same weights means that each site's impact estimate does not necessarily contribute one-eighth to the final impact estimate when we estimate impacts by subgroups.

Some sample regressions used to estimate program impacts are shown in Exhibit B.4

[6] This is accomplished by a two-stage regression procedure. The first stage runs the impact regression and calculates adjusted mean squared errors by site. The second stage reruns the regression, weighting by the inverse of this adjusted mean squared error. This correction adjusts for the possibility that the size of residuals is related to site.

Exhibit B.4

Sample Regressions Used to Estimate Impacts on Benefit Receipt

Dependent variable: Percentage of months of SSI receipt, months 1-30.

R-square	0.7967
Adj R-sq	0.7953
Dep Mean	0.35322

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > |T|
INTERCEP	1	0.231368	0.10218942	2.264	0.0236
TDAL	1	0.010306	0.01214704	0.848	0.3962
TFTW	1	-0.010908	0.01482040	-0.736	0.4618
TPHLV	1	0.017552	0.01241276	1.414	0.1574
TMN	1	-0.005980	0.01224041	-0.489	0.6252
TNH	1	-0.022685	0.01211375	-1.873	0.0612
TVA	1	0.021092	0.01189199	1.774	0.0762
TFL	1	-0.003706	0.01254322	-0.295	0.7676
TSPCD	1	-0.002854	0.01341164	-0.213	0.8315
DAL	1	-0.018958	0.01256172	-1.509	0.1313
FTW	1	-0.003156	0.01405993	-0.224	0.8224
MN	1	-0.008449	0.01290754	-0.655	0.5128
NH	1	-0.019595	0.01313102	-1.492	0.1357
PHLV	1	-0.020359	0.01271104	-1.602	0.1093
VA	1	0.015491	0.02920857	0.530	0.5959
SPCD	1	0.017888	0.01333250	1.342	0.1797
PDI0	1	0.001826	0.01667002	0.110	0.9128
PDI1YR	1	-0.025821	0.01189251	-2.171	0.0299
PDI2YR	1	0.009052	0.01009152	0.897	0.3698
PDI3YR	1	0.005494	0.01023999	0.537	0.5916
PDI45YR	1	-0.001316	0.00895913	-0.147	0.8833
PSI0	1	-0.116757	0.01110450	-10.514	0.0001
PSI1YR	1	-0.127592	0.01045473	-12.204	0.0001
PSI2YR	1	-0.057995	0.01094871	-5.297	0.0001
PSI3YR	1	-0.042286	0.01184347	-3.570	0.0004
PSI45YR	1	-0.019859	0.01067914	-1.860	0.0630
FEM	1	0.037204	0.00548071	6.788	0.0001
GENMISS	1	-0.046671	0.04691906	-0.995	0.3199
BLACK	1	-0.005614	0.01205602	-0.466	0.6415
WHITE	1	-0.012094	0.01105203	-1.094	0.2739
OTHRACE	1	0.019967	0.01601039	1.247	0.2124
SINGLE	1	-0.039480	0.09394447	-0.420	0.6743
DIV	1	-0.048675	0.09400897	-0.518	0.6046
MARRIEDM	1	-0.059396	0.09415884	-0.631	0.5282
MARRIEDF	1	-0.072445	0.09427595	-0.768	0.4423
MENTDIS	1	0.014932	0.00562082	2.657	0.0079
NEURDIS	1	0.013003	0.01021491	1.273	0.2031
MUSCDIS	1	0.002248	0.00771682	0.291	0.7708
DISMISS	1	-0.005319	0.00992726	-0.536	0.5921
RABESOL	1	-0.002013	0.00953036	-0.211	0.8327
RAAFSOL	1	0.007533	0.00640656	1.176	0.2397
AGE1830	1	0.014985	0.01553833	0.964	0.3349
AGE3145	1	-0.003604	0.01476819	-0.244	0.8072
AGE4659	1	0.003821	0.01477578	0.259	0.7960
DROPOUT	1	0.056142	0.02723680	2.061	0.0393
HSGRAD	1	0.038282	0.02703093	1.416	0.1567
VOC	1	0.037294	0.02729447	1.366	0.1719
BA	1	0.029729	0.02795025	1.064	0.2875
PERM	1	-0.026094	0.00870407	-2.998	0.0027
PERMISS	1	-0.015912	0.00659731	-2.412	0.0159

SPARTRA	1	0.634660	0.00785108	80.837	0.0001
PARTRA	1	0.005137	0.01791555	0.287	0.7743

Exhibit B.4 (Continued)

Dependent variable: Average Monthly SSI benefits, months 1-30.

R-square	0.8018
Adj R-sq	0.8005
Dep Mean	108.08

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > |T|
INTERCEP	1	61.883929	38.80340852	1.595	0.1108
TDAL	1	3.587180	4.55724991	0.787	0.4312
TFTW	1	0.737017	5.11209619	0.144	0.8854
TPHLV	1	0.624553	4.89389029	0.128	0.8985
TMN	1	-2.208177	4.65964216	-0.474	0.6356
TNH	1	-8.207139	4.71721283	-1.740	0.0819
TVA	1	3.933022	4.22220718	0.932	0.3516
TFL	1	-1.936356	4.54584387	-0.426	0.6701
TSPCD	1	-2.867480	5.90873139	-0.485	0.6275
DAL	1	-5.279932	4.62899930	-1.141	0.2541
FTW	1	-7.006463	4.95695077	-1.413	0.1576
MN	1	-2.222508	4.79929800	-0.463	0.6433
NH	1	-21.948628	4.94736704	-4.436	0.0001
PHLV	1	-8.917445	4.80583859	-1.856	0.0636
VA	1	-6.103276	10.66673891	-0.572	0.5672
SPCD	1	5.264289	5.38385244	0.978	0.3282
PDI0	1	4.809089	4.71512623	1.020	0.3078
PDI1YR	1	-16.399280	4.43001135	-3.702	0.0002
PDI2YR	1	0.724093	3.79386109	0.191	0.8486
PDI3YR	1	1.214820	3.85477732	0.315	0.7527
PDI45YR	1	-0.092465	3.37081322	-0.027	0.9781
PSI0	1	-21.004863	4.01191613	-5.236	0.0001
PSI1YR	1	-25.305192	3.93611695	-6.429	0.0001
PSI2YR	1	-12.586189	4.14544543	-3.036	0.0024
PSI3YR	1	-7.959466	4.49661346	-1.770	0.0767
PSI45YR	1	-0.922467	4.05677615	-0.227	0.8201
FEM	1	11.696197	2.07834976	5.628	0.0001
GENMISS	1	-33.302564	17.49516943	-1.904	0.0570
BLACK	1	-2.021347	4.56129317	-0.443	0.6577
WHITE	1	-3.028539	4.22387842	-0.717	0.4734
OTHRACE	1	9.888010	6.07549446	1.628	0.1037
SINGLE	1	-16.351997	36.28513625	-0.451	0.6523
DIV	1	-21.325422	36.31001213	-0.587	0.5570
MARRIEDM	1	-24.370688	36.36882963	-0.670	0.5028
MARRIEDF	1	-28.634850	36.40731647	-0.787	0.4316
MENTDIS	1	0.248465	2.11806497	0.117	0.9066
NEURDIS	1	-2.234519	3.84467876	-0.581	0.5611
MUSCDIS	1	-1.960127	2.92210728	-0.671	0.5024
DISMISS	1	-1.649457	3.74417492	-0.441	0.6596
RABESOL	1	0.909832	3.63748144	0.250	0.8025
RAAFSOL	1	1.056121	2.40425394	0.439	0.6605
AGE1830	1	0.144668	5.82906842	0.025	0.9802
AGE3145	1	-2.844572	5.53865576	-0.514	0.6076
AGE4659	1	0.169117	5.54914664	0.030	0.9757
DROPOUT	1	5.131920	9.98639200	0.514	0.6073
HSGRAD	1	0.572965	9.90406062	0.058	0.9539
VOC	1	0.658036	10.01216386	0.066	0.9476
BA	1	-2.707918	10.28685757	-0.263	0.7924
PERM	1	-11.928491	3.25950784	-3.660	0.0003
PERMISS	1	-1.708600	2.50429338	-0.682	0.4951

SSDIRA	1	-0.020077	0.00531655	-3.776	0.0002
SSIRA	1	0.694579	0.00764245	90.884	0.0001

Exhibit B.4 (Continued)

Dependent variable: Percentage of months of SSDI receipt, months 1-42.

R-square	0.8853
Adj R-sq	0.8845
Dep Mean	0.3104

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > |T|
INTERCEP	1	0.067511	0.06512297	1.037	0.2999
TDAL	1	-0.010145	0.01286998	-0.788	0.4306
TFTW	1	-0.018833	0.01536502	-1.226	0.2203
TPHLV	1	-0.017010	0.01283666	-1.325	0.1852
TMN	1	0.005251	0.01324880	0.396	0.6919
TNH	1	-0.003222	0.00469869	-0.686	0.4930
TVA	1	-0.008713	0.01224238	-0.712	0.4767
TFL	1	0.010834	0.01375915	0.787	0.4311
TSPCD	1	0.000547	0.01081578	0.051	0.9597
DAL	1	0.005319	0.01349735	0.394	0.6935
FTW	1	0.018880	0.01487962	1.269	0.2045
MN	1	0.007165	0.01397162	0.513	0.6081
NH	1	-0.035256	0.01101053	-3.202	0.0014
PHLV	1	0.031727	0.01351742	2.347	0.0189
VA	1	-0.006309	0.02266746	-0.278	0.7808
SPCD	1	0.001482	0.01272401	0.116	0.9073
PDI0	1	-0.035759	0.01600768	-2.234	0.0255
PDI1YR	1	-0.081183	0.01061138	-7.651	0.0001
PDI2YR	1	-0.037735	0.00939216	-4.018	0.0001
PDI3YR	1	-0.020428	0.00938645	-2.176	0.0296
PDI45YR	1	-0.009191	0.00820526	-1.120	0.2627
PSI0	1	0.036868	0.00782436	4.712	0.0001
PSI1YR	1	0.042373	0.00722541	5.864	0.0001
PSI2YR	1	0.005103	0.00842531	0.606	0.5448
PSI3YR	1	-0.013306	0.00906763	-1.467	0.1423
PSI45YR	1	-0.009908	0.00828792	-1.195	0.2319
FEM	1	0.003262	0.00419291	0.778	0.4366
GENMISS	1	-0.000066859	0.03211807	-0.002	0.9983
BLACK	1	0.011749	0.00792921	1.482	0.1384
WHITE	1	0.016059	0.00556209	2.887	0.0039
OTHRACE	1	-0.008854	0.01223108	-0.724	0.4692
SINGLE	1	-0.002286	0.05702201	-0.040	0.9680
DIV	1	-0.004003	0.05706745	-0.070	0.9441
MARRIEDM	1	0.014701	0.05721717	0.257	0.7972
MARRIEDF	1	-0.001271	0.05730802	-0.022	0.9823
MENTDIS	1	0.019792	0.00427198	4.633	0.0001
NEURDIS	1	0.030894	0.00757721	4.077	0.0001
MUSCDIS	1	0.012902	0.00577829	2.233	0.0256
DISMISS	1	0.003520	0.00802324	0.439	0.6609
RABESOL	1	0.009407	0.00697862	1.348	0.1777
RAAFSOL	1	-0.002134	0.00464360	-0.460	0.6458
AGE1830	1	-0.025288	0.01179718	-2.144	0.0321
AGE3145	1	-0.018865	0.01126941	-1.674	0.0942
AGE4659	1	-0.007995	0.01129121	-0.708	0.4789
DROPOUT	1	-0.034925	0.01898759	-1.839	0.0659
HSGRAD	1	-0.035121	0.01883550	-1.865	0.0623
VOC	1	-0.032891	0.01908832	-1.723	0.0849
BA	1	-0.021417	0.01954950	-1.096	0.2733
PERM	1	0.003005	0.00667083	0.451	0.6523
PERMISS	1	-0.008201	0.00455196	-1.802	0.0716

SPARTRA	1	0.043879	0.00556077	7.891	0.0001
PARTRA	1	0.860474	0.01565652	54.960	0.0001

Exhibit B.4 (Continued)

Dependent variable: Average Monthly SSDI Benefit, months 1-42.

R-square	0.7289
Adj R-sq	0.7272
Dep Mean	253.76

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > |T|
INTERCEP	1	-101.423858	78.22617932	-1.297	0.1948
TDAL	1	-13.889166	11.72555145	-1.185	0.2362
TFTW	1	-6.619581	14.92651455	-0.443	0.6574
TPHLV	1	-19.541339	11.79141994	-1.657	0.0975
TMN	1	4.032213	11.80780019	0.341	0.7327
TNH	1	-3.172800	6.83496671	-0.464	0.6425
TVA	1	-2.998973	11.26796694	-0.266	0.7901
TFL	1	12.127132	11.56886258	1.048	0.2946
TSPCD	1	5.991847	11.28189735	0.531	0.5954
DAL	1	34.736261	11.80320979	2.943	0.0033
FTW	1	26.969398	13.63110568	1.979	0.0479
MN	1	23.821955	12.13265401	1.963	0.0496
NH	1	-31.315913	10.26232302	-3.052	0.0023
PHLV	1	40.474056	11.86973336	3.410	0.0007
VA	1	28.197665	24.65337338	1.144	0.2528
SPCD	1	11.812033	11.74061779	1.006	0.3144
PDI0	1	-22.888602	15.35079640	-1.491	0.1360
PDI1YR	1	-31.631617	10.19797596	-3.102	0.0019
PDI2YR	1	0.042325	9.15065999	0.005	0.9963
PDI3YR	1	23.087965	9.24667170	2.497	0.0125
PDI45YR	1	11.195948	8.09079462	1.384	0.1665
PSI0	1	101.345887	8.56804409	11.828	0.0001
PSI1YR	1	97.324519	7.98561665	12.187	0.0001
PSI2YR	1	36.275459	9.23446581	3.928	0.0001
PSI3YR	1	13.939848	9.99696046	1.394	0.1632
PSI45YR	1	6.843952	9.07458472	0.754	0.4508
FEM	1	-22.166776	4.64226916	-4.775	0.0001
GENMISS	1	-46.512689	37.82520139	-1.230	0.2189
BLACK	1	37.284171	8.76740739	4.253	0.0001
WHITE	1	68.097401	7.18732383	9.475	0.0001
OTHRACE	1	20.585376	12.88930835	1.597	0.1103
SINGLE	1	27.633814	70.57947160	0.392	0.6954
DIV	1	36.646017	70.63309879	0.519	0.6039
MARRIEDM	1	87.661898	70.77979172	1.239	0.2156
MARRIEDF	1	9.030531	70.88549532	0.127	0.8986
MENTDIS	1	-0.815435	4.74416815	-0.172	0.8635
NEURDIS	1	14.192312	8.54725917	1.660	0.0969
MUSCDIS	1	-6.348192	6.47834312	-0.980	0.3272
DISMISS	1	-4.851038	8.59266526	-0.565	0.5724
RABESOL	1	-1.548815	7.88705009	-0.196	0.8443
RAAFSOL	1	-10.567242	5.28574987	-1.999	0.0456
AGE1830	1	-8.771342	13.12800824	-0.668	0.5041
AGE3145	1	7.909533	12.50458267	0.633	0.5271
AGE4659	1	42.691062	12.52070228	3.410	0.0007
DROPOUT	1	-14.655470	22.41532323	-0.654	0.5132
HSGRAD	1	-12.034082	22.24793026	-0.541	0.5886
VOC	1	-0.004463	22.49183529	-0.000	0.9998
BA	1	37.739526	23.01711081	1.640	0.1011
PERM	1	21.543892	7.37917206	2.920	0.0035

PERMISS	1	38.679522	5.22546912	7.402	0.0001
SPARTRA	1	-16.514925	6.17330394	-2.675	0.0075
PARTRA	1	452.008102	15.03530618	30.063	0.0001

Exhibit B.4 (Continued)

Definitions of variables

TDAL,TFTW,TPHLV,TMN,TNH,TVA,TFL,TSPCD: dummy variables set equal to 1 if sample member is in the treatment group and resides in, respectively, Dallas, Fort Worth, Phoenix/Las Vegas, Minneapolis, New Hampshire, Richmond, Tampa, Spokane/Coeur d'Alene

DAL,FTW,PHLV,MN,NH,VA,FL,SPCD: dummy variables set equal to 1 if sample member resides in, respectively, Dallas, Fort Worth, Phoenix/Las Vegas, Minneapolis, New Hampshire, Richmond, Tampa, Spokane/Coeur d'Alene (Florida dummy is omitted in the regressions).

PDI0, PDI1YR, PDI2YR, PDI3YR, PDI45YR: dummy variables set equal to 1 if sample member received SSDI benefits before random assignment for, respectively, 0 months, 1-12 months, 13-24 months, 25-36 months, 37-60 months (dummy for more than 60 months is omitted).

PSI0, PSI1YR, PSI2YR, PSI3YR, PSI45YR: dummy variables set equal to 1 if sample member received SSDI benefits before random assignment for, respectively, 0 months, 1-12 months, 13-24 months, 25-36 months, 37-60 months (dummy for more than 60 months is omitted).

FEM, GENMISS:dummy variables set equal to 1 if sample member is, respectively, female or if gender data are missing (dummy for male is omitted).

BLACK, WHITE, OTHRACE:dummy variables set equal to 1 if sample member is, respectively,African American, White, or other race (dummy for missing data is omitted).

SINGLE, DIV, MARRIEDM,MARRIEDF:dummy variables set equal to 1 if sample member is, respectively, single, divorced/widowed/separated,missing data, married and male, married and female (dummy for missing data is omitted)

MENTDIS, NEURDIS, MUSCDIS, DISMISS: dummy variables set equal to 1 if sample member's primary impairment is, respectively, mental, neurological, musculoskeletal, missing data (dummy for other impairment is omitted)

RABESOL, RAAFSOL:dummy variables set equal to 1 if sample member was randomly assigned, respectively, 1 month before solicitation or more than 6 months after solicitation (dummy for remaining group is omitted).

AGE1830, AGE3145, AGE4659: dummy variables set equal to 1 if sample member's age at random assignment was, respectively, 18-30 years, 31-45 years, 46-59 years (dummy for those age 60 and above is omitted).

DROPOUT, HSGRAD, VOC, BA: dummy variables set equal to 1 if sample member's highest level of schooling attained at random assignment was, respectively, less than a high school diploma or GED, high school diploma or GED, some additional vocational training, or a four-year college degree (dummy for missing data is omitted).

PERM, PERMISS:dummy variables set equal to 1 if sample member's disability is coded in the SSA administrative data as, respectively, permanent or data missing (dummy for coded as not permanent) is omitted)

SPARTRA, PARTRA: dummy variables set equal to 1 if the sample member received, respectively, SSI or SSDI benefits at random assignment

SSIRA, SSDIRA: value of monthly SSI or SSDI benefit at random assignment

B.5.2 Estimating impacts on outcomes measured by the follow-up survey

The procedure used to estimate impacts on survey-based outcomes for the subgroup of follow-up survey respondents is very similar to the procedure used to estimate impacts on SSI and SSDI benefit receipt for the full sample. The impact estimates are regression-adjusted estimates of differences in mean outcomes between treatment and control groups. The independent variables in these regressions are those used in the estimation of impacts on benefit receipt and those obtained in the baseline survey.

The sampling weights used for the survey sample differ from those used for the full sample, because our survey sampling strategy over-sampled persons solicited as young SSI recipients (age 30 years and below). The survey sampling weights must therefore "weight down" the young SSI recipient sample and "weight up" the remaining sample. The survey sampling weights ensure that within each of the 8 sites, the young SSI sample constitutes the same weighted proportion of both the survey sample and the full sample of NetWork volunteers. We also weight observations so that each of the 8 sites contributes one-eighth of the total weighted sample.

For each outcome, we estimate impacts using two regressions, one for the young SSI sample and a second for the rest of the sample. In each, we regress the outcome on 8 treatment dummies, one for each site, 7 site dummies, and the independent variables. We include the same site-level heteroskedasticity correction used for the full sample. The final overall impact estimate is a weighted average of the 16 impact estimates obtained from these 2 regressions. Similarly, the variance of this final estimate is the weighted average of the variance of these 16 impact estimates.

In estimating impacts for subgroups defined by primary impairment or title of eligibility, we use only the sample of persons belonging to the subgroup. These same person-weights that were used to estimate impacts for the overall sample were used to produce impact estimates for these subgroups. Because the proportion of persons who fall into these subgroups varies by site, using the same weights means that each site's impact estimate does not necessarily contribute one-eighth to the final impact estimate when we estimate impacts by subgroups.

B.5.3 Estimating Impacts on Annual Earnings using Administrative Data

The most important source of data on earnings is a set of administrative data on annual (calendar year) earnings from the Master Earnings File (MEF). These data were obtained by the Social Security Administration for all sample members. Earnings records from this source are available through calendar year 1996. Because of rules regarding the confidentiality of earnings data, only staff at SSA/ORES were allowed to view and analyze these earnings data.

Because impacts may vary according to time elapsed since random assignment, we converted these records of calendar year earnings to "follow-up year" earnings. We defined earnings in follow-up year 1 as earnings in the first full calendar year after random assignment, which occurred from mid-1992 through mid-1994. The "first follow-up year" is therefore calendar year 1993 for those randomly assigned in 1992; calendar year 1994 for those randomly assigned in 1993; and calendar year 1995 for those randomly assigned in 1994. Thus, our measure of average

earnings in the "first follow-up year" includes some earnings obtained after the first twelve months following random assignment. All sample members have at least two follow-up years of earnings; those randomly assigned in 1992 and 1993 have a third follow-up year of earnings.

The values of average annual earnings for treatment and control group members are based on simple, unweighted calculations of means without regression adjustment. The estimated impacts and associated tests of statistical significance are based on simple, unweighted comparisons of these means (and variances), without regression adjustment.

B. 6 Supplementary Exhibit on Service Receipt, by Type of Primary Impairment

Exhibit B.5 presents an additional analysis of service receipt, by impairment group.

Exhibit B.5
Treatment/Control Differences in Reported Receipt of Services, by Primary Impairment

	Mental		Neurological		Musculoskeletal		Other	
Service	Control Group Mean	Impact	Control Group Mean	Impact	Control Group Mean	Impact	Control Group Mean	Impact
Job Search Assistance	16.2%	9.0**	14.2%	5.0	9.8%	11.2	12.2%	6.1*
Business skills training	6.7	3.2	0.0	14.0	5.3	11.7*	6.1	4.0
Job-related training	11.1	3.5	14.9	-11.2	10.3	-0.9	9.0	2.3
Other rehabilitaion/training	4.0	-1.0	3.6	-3.8	1.1	-0.9	0.0	0.4
Life skills training	6.9	2.3	6.0	2.5	1.1	3.2	5.4	-2.8
Occupational therapy	4.4	-1.7	5.5	1.8	4.2	0.4	3.9	-0.1
College classes	10.8	-2.0	9.1	-4.3	14.1	-6.4	8.1	0.4
Assessment of Work Potential	15.4	10.5***	30.9	-17.9*	12.2	16.4**	18.7	11.1***
Physical Therapy	13.6	2.1	24.6	37.9***	29.9	13.2	26.0	-5.9
Psychological Counseling	60.0	5.3	36.8	-29.0**	18.5	10.3	23.7	2.2
Any service	80.4	2.8	66.1	5.4	58.4	13.4	62.7	5.5

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
Mental Impairment: 334 persons in the treatment group, 305 in the control group, 639 in total
Neurological Impairment: 52 persons in the treatment group, 49 in the control group, 101 in total
Musculoskeletal Impairment: 100 persons in the treatment group, 82 in the control group, 182 in total
Other Impairment: 300 persons in the treatment group, 299 in the control group, 599 in total

Source: Follow-up Survey

Appendix C
Additional Employment and Earnings Results

This appendix provides additional results from the analysis of impacts on employment and earnings presented in Chapter 4.

Exhibit C.1
Impacts on Average Annual Earnings, by Site and Program Model

Average Annual Earnings, Follow-up Years 1-2	Average Annual Earnings, Control Group	Impact	Standard Error
Model 1	$2,239	$147	$235
Dallas	2,308	320	318
Fort Worth	2,130	-103	339
F-test, differences in impact between sites		*	
Model 2	$1,930	$300	$184
Phoenix/Las Vegas	1,734	357	279
Minneapolis	2,161	208	244
F-test, differences in impact between sites		n.s.	
Model 3	$1,864	$538***	$197
New Hampshire	1,904	662**	273
Richmond	1,828	409	284
F-test, differences in impact between sites		n.s.	
Model 4	$1,718	$-133	$174
Tampa	1,883	61	254
Spokane/Couer d'Alene	1,541	-391*	234
F-test, differences in impact between sites		**	

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Dallas: 570 in the treatment group, 577 in the control group, 1,147 in total
Fort Worth: 386 in the treatment group, 366 in the control group, 752 in total
Phoenix/Las Vegas: 545 in the treatment group 555 in the control group,1,100 in total
Minneapolis: 543 in the treatment group, 469 in the control group, 1,012 in total
New Hampshire: 545 in the treatment group, 538 in the control group, 1,083 in total
Richmond: 542 in the treatment group, 589 in the control group, 1,131 in total
Tampa: 564 in the treatment group, 515 in the control group, 1,079 in total
Spokane/Couer d'Alene: 465 in the treatment group,479 in the control group, 944 in total

Earnings data are available for calendar years only, from 1990-1996. Random assignment occurred between mid-1992 to mid-1994. "Follow-up year 1" is defined as the first full calendar year after the month of random assignment. For those randomly assigned in 1994, only 2 follow-up years of earnings data are available.

Earnings data are annual earnings data provided by SSA/ORES from the Master Earnings File (MEF)

Exhibit C.2
Impacts on Average Annual Earnings, by Title of Eligibility, Based on Follow-up Survey

	Average Annual Earnings, Control Group	Impact	Standard Error
	SSI Only		
Year 1	$509	$38	$209
Year 2	$896	-$33	$290
Year 1-2	$701	$5	$233
Latest Year	$1,121	-$111	$314
	SSDI Only		
Year 1	$986	$116	$247
Year 2	$1,615	$457	$352
Year 1-2	$1,306	$275	$276
Latest Year	$1,928	$270	$368
	Both SSI and SSDI		
Year 1	$442	$20	$230
Year 2	$1,419	-$349	$531
Year 1-2	$924	-$148	$332
Latest Year	$1,640	-$65	$609
	Neither SSI or SSDI		
Year 1	$2,173	$107	$602
Year 2	$3,170	$962	$769
Year 1-2	$2,680	$511	$629
Latest Year	$3,892	$396	$811

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
SSI only: 246 in the treatment group, 203 in the control group, 449 total.
SSDI only: 304 in the treatment group, 304 in the control group, 608 total.
Both SSI and SSDI: 111 in the treatment group, 103 in the control group, 214 total.
Neither SSI nor SSDI: 125 in the treatment group, 125 in the control group, 250 total.

Source: Follow-up Survey

Exhibit C.3
Impacts on Average Annual Earnings, by Primary Impairment, Based on Follow-up Survey

	Average Annual Earnings, Control Group	Impact	Standard Error
	Mental Impairment		
Year 1	$943	-$234	$208
Year 2	$1,643	$24	$320
Year 1-2	$1,293	-$105	$236
Latest Year	$1,957	-$83	$346
	Neurological Impairment		
Year 1	$1,622	-$1,456**	$574
Year 2	$1,838	-$527	$994
Year 1-2	$1,715	-$952	$672
Latest Year	$2,261	-1,609	$1,033
	Musculoskeletal Impairment		
Year 1	$612	$290	$515
Year 2	$800	1,015	$696
Year 1-2	$677	$705	$571
Latest Year	$1,304	$1,261	$882
	Other Impairments		
Year 1	$1,223	$232	$288
Year 2	$1,852	$588	$391
Year 1-2	$1,545	$392	$316
Latest Year	$2,042	$675*	$405

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Mental impairment: 334 in the treatment group, 305 in the control group, 639 total.
Neurological impairment: 52 in the treatment group, 49 in the control group, 101 total.
Musculoskeletal impairment: 100 in the treatment group, 82 in the control group, 182 total.
Other impairment: 300 in the treatment group, 299 in the control group, 599 total.

Source: Follow-up Survey

Exhibit C.4
Impacts on Annual Earnings, by Gender and Follow-up Year,
Based on Administrative Records

Follow-up Period	Average Annual Earnings, Control Group	Impact	Standard Error
	Men		
Year 1	$1,863	$254*	$132
Year 2	2,218	302*	159
Years 1-2	2,041	278*	137
	Women		
Year 1	$1,612	$164	$136
Year 2	1,952	119	161
Years 1-2	1,782	141	140
F-test, difference in impacts among program subgroups			
Year 1		ns	
Year 2		ns	
Years 1-2		ns	

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Men: 2,398 in the treatment group, 2,364 in the control group, 4,762 total
Women: 1,762 in the treatment group, 1,724 in the control group, 3,486 total

Earnings data are available for calendar years only, from 1990-1996. Random assignment occurred between mid-1992 to mid-1994. “Follow-up year 1” is defined as the first full calendar year after the month of random assignment. For those randomly assigned in 1994, only 2 follow-up years of earnings data are available.

Earnings data are annual earnings data provided by SSA/ORES from the Master Earnings File (MEF)

Exhibit C.5
Impacts on Annual Earnings, by Age at Random Assignment and Follow-up Year, Based on Administrative Records

Follow-up Period	Average Annual Earnings, Control Group	Impact	Standard Error
	Age 18-30 at Random Assignment		
Year 1	$1,895	$164	$182
Year 2	2,480	-5	226
Years 1-2	2,187	80	190
	Age 31-45 at Random Assignment		
Year 1	$1,925	$244	$149
Year 2	2,290	334*	182
Years 1-2	2,108	289*	157
	Age 46 and over at Random Assignment		
Year 1	$1,415	$221	$165
Year 2	1,577	238	186
Years 1-2	1,496	229	165
F-test, difference in impacts among program subgroups			
Year 1		ns	
Year 2		ns	
Years 1-2		ns	

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes: of persons analyzed for this exhibit are as follows:

Age 18-30 at random assignment: 925 in the treatment group, 886 in the control group, 1,811 in total
Age 31-45 at random assignment: 1,893 in the treatment group, 1,909 in the control group, 3,802 in total
Age 46 and over at random assignment: 1,342 in the treatment group, 1,293 in the control group, 2,635 in total

Earnings data are available for calendar years only, from 1990-1996. Random assignment occurred between mid-1992 to mid-1994. “Follow-up year 1” is defined as the first full calendar year after the month of random assignment. For those randomly assigned in 1994, only 2 follow-up years of earnings data are available.

Earnings data are annual earnings data provided by SSA/ORES from the Master Earnings File (MEF)

Exhibit C.6
Impacts on Annual Earnings, by Length of Time Receiving Benefits and Follow-up Year, Based on Administrative Records

Follow-up Period	Average Annual Earnings, Control Group	Impact	Standard Error
Received SSDI and/or SSI less than 3 years at random assignment			
Year 1	$1,651	$172	$168
Year 2	1,886	278	197
Years 1-2	1,786	225	172
Received SSDI and/or SSI more than 3 years at random assignment			
Year 1	$1,213	$237**	$116
Year 2	1,508	144	141
Years 1-2	1,360	190	122
F-test, difference in impacts among program subgroups			
Year 1		ns	
Year 2		ns	
Years 1-2		ns	

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes :

Received SSI and/or SSDI less than 3 years at random assignment: 1,328 in the treatment group, 1,295 in the control group, 2,623 in total
Received SSI and/or SSDI more than 3 years at random assignment: 1,891 in the treatment group, 1,864 in the control group, 3,755 in total

Samples include only those who received SSI and/or SSDI at random assignment.
Earnings data are available for calendar years only, from 1990-1996. Random assignment occurred between mid-1992 to mid-1994. “Follow-up year 1” is defined as the first full calendar year after the month of random assignment. For those randomly assigned in 1994, only 2 follow-up years of earnings data are available.

Earnings data are annual earnings data provided by SSA/ORES from the Master Earnings File (MEF)

Exhibit C.7
Impacts on Employment, by Title of Eligibility
Based on Follow-up Survey

	SSI Only			SSDI Only			Both SSI and SSDI			Neither SSI nor SSDI			Neither SSI nor SSDI: New SSI Applicants		
Follow-up Period	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error
							Total Hours Worked								
Year 1	102	13	40	180	27	42	106	-12	47	363	24	101	366	41	115
Year 2	164	40	55	276	86	54	244	-42	86	580	129	140	597	188	164
Year 1-2	265	55	88	459	107	89	348	-47	116	950	140	220	969	219	254
Latest Year	193	34	57	342	32	57	281	19	98	708	-16	137	732	28	162
							Avg. # of Months Employed								
Year 1	.9	.3	.3	1.3	.3	.3	1.1	-.3	.4	2.3	.4	.6	2.3	0.4	0.7
Year 2	1.3	.5	.4	2.1	.7*	.4	2.0	-.3	.6	3.4	1.0	.7	3.4	1.3	0.8
Year 1-2	2.2	.8	.7	3.4	1.0*	.6	3.1	-.6	1.0	5.7	1.3	1.2	5.7	1.6	1.4
Latest Year	1.6	.6	.4	2.7	.2	.4	2.4	.1	.7	4.0	.8	.7	4.0	0.8	0.8

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
SSI only: 246 in the treatment group, 203 in the control group, 449 total.
SSDI only: 304 in the treatment group, 304 in the control group, 608 total.
Both SSI and SSDI: 111 in the treatment group, 103 in the control group, 214 total.
Neither SSI nor SSDI: 125 in the treatment group, 125 in the control group, 250 total.
Neither SSI nor SSDI: New SSI Applicants: 113 in the treatment group, 103 in the control group, 216 total.

Source: Follow-up Survey

Exhibit C.8
Impacts on Employment, by Primary Impairment
Based on Follow-up Survey

	Mental			Neurological			Musculoskeletal			Other		
Follow-up Period	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error
					Total Hours Worked							
Year 1	194	-52	38	191	-103	98	160	-28	92	212	40	49
Year 2	297	17	53	234	28	137	267	42	134	343	55	68
Year 1-2	492	-38	82	420	-64	209	419	28	203	558	86	108
Latest Year	349	-6	56	385	-252	159	344	43	139	394	27	66
					Avg. # of Months Employed							
Year 1	1.5	-.2	.3	1.4	-.7	.8	.9	.2	.6	1.4	.4	.3
Year 2	2.3	.4	.4	1.6	.5	1.0	1.5	.4	.7	2.2	.7*	.4
Year 1-2	3.9	.2	.6	2.9	-.0	1.6	2.5	.5	1.3	3.7	1.1*	.7
Latest Year	2.7	.3	.4	2.1	-.5	1.1	1.9	.7	.8	2.6	.7	.4

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
Mental impairment: 334 in the treatment group, 305 in the control group, 639 total.
Neurological impairment: 52 in the treatment group, 49 in the control group, 101 total.
Musculoskeletal impairment: 100 in the treatment group, 82 in the control group, 182 total.
Other impairment: 300 in the treatment group, 299 in the control group, 599 total.

Source: Follow-up Survey

Exhibit C.9
Impacts on Wage Rates and Fringe Benefits--
Full Sample of Follow-up Survey Respondents

	Control Group Mean	Impact	Standard Error
Average Hourly Wage in Follow-up Month 24			
Above $6.50 in Month 24	8%	3%	2
Below $6.50 in Month 24	11	3	2
Fringe Benefits, Current/Most Recent Job			
Health insurance	10%	1%	2
Dental insurance	9	-1	2
Paid Sick Leave	9	1	2
Paid Vacation	14	-1	2
Pension/retirement	8	-0	2

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level
Outcomes and impacts are expressed as the percent of sample members with hourly wages falling within the specified range, or with the specified type of fringe benefit.
Sample sizes:
786 persons in the treatment group, 735 in the control group, 1,521 in total

Source: Follow-up Survey

Exhibit C.10
Impacts on Wage Rates and Fringe Benefits, by Primary Impairment

	Mental			Neurological			Musculoskeletal			Other		
	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error
Average Hourly Wage in Follow-up Month 24												
Above $6.50 in Month 24	8%	-1%	3	6%	4%	7	3%	7%	5	9%	6%**	3
Below $6.50 in Month 24	10	7**	3	10	-0	7	8	1	6	11	2	3
Fringe Benefits, Current/Most Recent Job												
Health insurance	10%	0%	3	12%	-11%	8	10%	-0%	6	11%	2%	3
Dental insurance	7	1	2	12	-11*	6	11	-6	5	11	-2	3
Paid Sick Leave	10	-1	3	17	-16*	1	11	-2	5	8	2	3
Paid Vacation	16	-5	3	14	-12	9	18	-8	7	14	2	3
Pension/retirement	8	-3	2	12	-11*	6	8	-3	5	9	2	3

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level
Sample sizes:
Mental impairment: 334 in the treatment group, 305 in the control group, 639 total.
Neurological impairment: 52 in the treatment group, 49 in the control group, 101 total.
Musculoskeletal impairment: 100 in the treatment group, 82 in the control group, 182 total.
Other impairment: 300 in the treatment group, 299 in the control group, 599 total.

Source: Follow-up survey

Exhibit C.11
Impacts on Wage Rates and Fringe Benefits, by Title of Eligibility

	SSI Only			SSDI Only			Both SSI and SSDI			Neither SSI nor SSDI			Neither SSI nor SSDI, SSI Applicants		
	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error	Control Group Mean	Impact	Standard Error
Average Hourly Wage in Follow-up Month 24															
Above $6.50 in Month 24	5%	1%	2	9%	2%	3	7%	-2%	4	15%	9%*	6	14%	11*	6
Below $6.50 in Month 24	8	.02	.03	11	3	3	10	7	6	15	3	5	17	0	6
Fringe Benefits, Current/Most Recent Job															
Health insurance	4%	1%	2	10%	4%	3	8%	-0%	4	23%	-5%	6	22%	-2%	6
Dental insurance	3	1	2	9	-0	3	5	3	4	20	-7	5	20	-7	6
Paid Sick Leave	5	-1	2	8	4	3	8	1	4	17	-3	6	14	3	6
Paid Vacation	7	-1	3	13	3	3	13	-2	5	30	-9	6	30	-8	7
Pension/retirement	3	-0	2	6	3	2	7	-3	4	21	-8	6	21	-6	6

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
SSI only: 246 in the treatment group, 203 in the control group, 449 total.
SSDI only: 304 in the treatment group, 304 in the control group, 608 total.
Both SSI and SSDI: 111 in the treatment group, 103 in the control group, 214 total.
Neither SSI nor SSDI: 125 in the treatment group, 125 in the control group, 250 total.
Neither SSI nor SSDI: New SSI Applicants: 113 in the treatment group, 103 in the control group, 216 total.

Source: Follow-up Survey

Appendix D
Additional Benefits Impacts Results

This appendix provides additional results from the analysis of impacts on SSI and SSDI benefit receipt presented in Chapter 5.

Exhibit D.1
Impacts on SSI Participation Rates -- Those Receiving only SSI at Random Assignment

Months Since Random Assignment	Control Group Mean	Treatment Group Mean	Impact	(Standard Error)
Month 1	98.3	98.2	-.1	.6
Month 2	96.8	96.8	.1	.7
Month 3	95.6	95.2	-.4	.9
Month 4	94.2	93.2	-1.0	1.0
Month 5	92.8	92.1	-.7	1.1
Month 6	91.4	90.5	-.9	1.2
Month 7	90.0	90.2	.2	1.2
Month 8	90.0	89.3	-.7	1.2
Month 9	89.8	89.0	-.7	1.3
Month 10	89.0	88.5	-.5	1.3
Month 11	87.7	88.2	.5	1.3
Month 12	88.2	87.8	-.4	1.3
Month 13	86.9	87.2	.3	1.4
Month 14	86.8	86.3	-.5	1.4
Month 15	85.4	85.3	-.2	1.5
Month 16	84.3	84.6	.2	1.5
Month 17	84.3	83.8	-.5	1.5
Month 18	84.3	84.2	-.1	1.5
Month 19	83.9	84.5	.6	1.5
Month 20	82.5	83.2	.7	1.6
Month 21	83.0	83.4	.4	1.5
Month 22	83.2	83.3	.1	1.5
Month 23	82.8	83.1	.3	1.5
Month 24	82.6	83.1	.4	1.6
Month 25	82.8	82.4	-.4	1.6
Month 26	82.3	82.6	.3	1.6
Month 27	81.6	82.2	.6	1.6
Month 28	82.2	81.4	-.8	1.6
Month 29	81.2	80.2	-1.0	1.6
Month 30	81.2	79.7	-1.5	1.6

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Received SSI at random assignment: 1,096 in the treatment group, 1,064 in the control group, 2,160 in total

These results are shown graphically in Exhibit 5.3.

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.2

Impacts on SSDI Participation Rates -- Those Receiving only SSDI at Random Assignment

Months Since Random Assignment	Control Group Mean	Treatment Group Mean	Impact	(Standard Error)
Month 1	99.8	99.7	-.1	.2
Month 2	99.7	99.5	-.2	.2
Month 3	99.2	99.2	-.0	.3
Month 4	99.0	98.7	-.3	.4
Month 5	98.8	98.4	-.4	.4
Month 6	98.2	98.2	-.1	.5
Month 7	98.0	97.8	-.2	.5
Month 8	97.8	97.2	-.5	.6
Month 9	97.7	97.1	-.6	.6
Month 10	97.6	96.5	-1.1*	.6
Month 11	97.2	96.1	-1.1*	.6
Month 12	96.9	95.9	-1.0	.7
Month 13	96.5	95.5	-1.0	.7
Month 14	96.0	95.1	-.9	.7
Month 15	95.3	94.5	-.8	.8
Month 16	94.9	94.4	-.4	.8
Month 17	94.5	93.8	-.7	.8
Month 18	94.0	93.3	-.7	.9
Month 19	93.8	92.8	-1.0	.9
Month 20	93.4	92.4	-1.0	.9
Month 21	92.9	92.0	-.9	.9
Month 22	92.7	91.3	-1.3	1.0
Month 23	92.1	90.9	-1.3	1.0
Month 24	91.6	90.5	-1.1	1.0
Month 25	90.4	90.3	-.1	1.1
Month 26	89.5	89.6	.0	1.1
Month 27	89.0	89.1	.1	1.1
Month 28	88.5	88.5	-.0	1.1
Month 29	88.3	87.5	-.8	1.2
Month 30	87.4	86.8	-.6	1.2
Month 31	87.1	86.3	-.8	1.2
Month 32	86.4	85.4	-1.0	1.2
Month 33	85.9	84.8	-1.1	1.3
Month 34	85.6	84.5	-1.0	1.3
Month 35	85.6	84.4	-1.3	1.3
Month 36	85.2	84.3	-1.0	1.3
Month 37	84.7	84.1	-.6	1.3
Month 38	84.4	83.7	-.6	1.3
Month 39	83.9	83.2	-.7	1.3
Month 40	83.7	82.7	-1.0	1.3
Month 41	83.6	82.4	-1.2	1.3
Month 42	83.2	81.8	-1.5	1.3

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
Received SSDI at random assignment: 1,570 in the treatment group, 1,556 in the control group, 3,136 in total
These results are shown graphically in Exhibit 5.3.

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.3
Impacts on SSI Participation Rates -- Those Receiving Both SSI and SSDI at Random Assignment

Months Since Random Assignment	Control Group Mean	Treatment Group Mean	Impact	(Standard Error)
Month 1	94.7	95.2	.5	1.3
Month 2	90.8	92.8	2.0	1.6
Month 3	86.3	91.1	4.7**	1.9
Month 4	85.5	89.5	4.0**	1.9
Month 5	83.7	87.3	3.6*	2.1
Month 6	81.7	86.1	4.4**	2.2
Month 7	80.9	84.1	3.3	2.3
Month 8	81.4	81.7	.4	2.4
Month 9	81.4	81.6	.2	2.4
Month 10	80.5	80.3	-.2	2.4
Month 11	80.5	79.1	-1.4	2.4
Month 12	79.5	79.4	-.1	2.4
Month 13	79.6	79.5	-.0	2.4
Month 14	76.9	78.3	1.4	2.5
Month 15	76.3	77.5	1.2	2.5
Month 16	74.7	77.5	2.7	2.6
Month 17	73.4	77.3	3.9	2.6
Month 18	72.1	76.6	4.6*	2.6
Month 19	72.9	76.6	3.8	2.6
Month 20	70.2	76.8	6.7**	2.6
Month 21	71.2	76.0	4.7*	2.6
Month 22	71.1	74.4	3.3	2.6
Month 23	69.9	75.9	5.9**	2.6
Month 24	69.9	75.1	5.3**	2.6
Month 25	71.0	73.6	2.5	2.6
Month 26	70.6	72.8	2.2	2.6
Month 27	69.1	73.0	3.8	2.6
Month 28	68.6	72.8	4.2	2.7
Month 29	68.6	72.4	3.8	2.6
Month 30	68.4	71.8	3.5	2.7

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
Received SSI and SSDI at random assignment: 553 in the treatment group, 539 in the control group, 1,092 in total

These results are shown graphically in Exhibit 5.3.

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.4
Impacts on SSDI Participation Rates -- Those Receiving Both SSI and SSDI at Random Assignment

Months Since Random Assignment	Control Group Mean	Treatment Group Mean	Impact	(Standard Error)
Month 1	99.8	100	.2**	.1
Month 2	99.8	99.8	-.0	.1
Month 3	99.8	99.6	-.2	.2
Month 4	99.6	99.8	.2	.2
Month 5	99.6	99.7	.1	.3
Month 6	99.4	99.5	.1	.3
Month 7	99.1	99.3	.2	.5
Month 8	98.7	99.2	.4	.5
Month 9	98.8	98.6	-.2	.7
Month 10	98.8	98.6	-.2	.7
Month 11	98.5	98.6	.0	.7
Month 12	98.1	98.4	.3	.7
Month 13	97.2	98.0	.8	.9
Month 14	96.7	97.8	1.1	1.0
Month 15	96.2	97.8	1.7	1.0
Month 16	96.0	97.2	1.2	1.1
Month 17	95.7	97.2	1.4	1.1
Month 18	95.1	96.8	1.7	1.2
Month 19	94.7	96.1	1.4	1.3
Month 20	94.4	95.5	1.1	1.3
Month 21	94.2	95.0	.8	1.3
Month 22	94.0	95.0	1.0	1.3
Month 23	93.4	95.1	1.7	1.4
Month 24	93.1	94.1	1.1	1.5
Month 25	93.0	93.8	.8	1.5
Month 26	92.7	93.6	.9	1.5
Month 27	92.5	93.0	.6	1.5
Month 28	92.3	93.2	1.0	1.5
Month 29	91.9	93.2	1.3	1.6
Month 30	90.5	92.5	2.0	1.7
Month 31	89.8	92.0	2.2	1.7
Month 32	88.8	92.6	3.8**	1.7
Month 33	88.5	92.2	3.7**	1.8
Month 34	88.0	90.8	2.8	1.8
Month 35	88.0	90.2	2.2	1.9
Month 36	87.6	89.9	2.3	1.9
Month 37	87.1	89.3	2.2	1.9
Month 38	87.1	89.0	1.9	1.9
Month 39	86.6	89.4	2.8	2.0
Month 40	85.5	88.8	3.3	2.0
Month 41	85.7	88.3	2.6	2.0
Month 42	84.9	87.8	2.9	2.1

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
Received SSI and SSDI at random assignment: 553 in the treatment group, 539 in the control group, 1,092 in total
These results are shown graphically in Exhibit 5.3.

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.5
Impacts on SSI Receipt, by Site and Program Model

Percentage of Months Receiving SSI, Months 1-30	Control Group	Impact	Standard Error
Model 1	32.7	-0.0	1.0
Dallas	30.7	1.0	1.2
Fort Worth	34.8	-1.1	1.5
Model 2	35.8	.6	.9
Phoenix/Las Vegas	33.0	1.8	1.2
Minneapolis	38.7	-.6	1.2
Model 3	32.1	-.1	.8
New Hampshire	30.2	-2.3*	1.2
Richmond	34.0	2.1*	1.2
Model 4	41.4	-.3	.9
Tampa	35.3	-.4	1.3
Spokane/Couer d'Alene	47.6	-.3	1.3

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Dallas: 570 in the treatment group, 577 in the control group, 1,147 in total
Fort Worth: 386 in the treatment group, 366 in the control group, 752 in total
Phoenix/Las Vegas: 545 in the treatment group 555 in the control group,1,100 in total
Minneapolis: 543 in the treatment group, 469 in the control group, 1,012 in total
New Hampshire: 545 in the treatment group, 538 in the control group, 1,083 in total
Richmond: 542 in the treatment group, 589 in the control group, 1,131 in total
Tampa: 564 in the treatment group, 515 in the control group, 1,079 in total
Spokane/Couer d'Alene: 465 in the treatment group,479 in the control group, 944 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.6
Impacts on SSDI Receipt, by Site and Program Model

Percentage of Months Receiving SSDI, Months 1-42	Control Group	Impact	Standard Error
Model 1	56.4	-1.4	1.0
Dallas	56.0	-1.0	1.3
Fort Worth	56.8	-1.9	1.5
Model 2	58.8	-.6	.9
Phoenix/Las Vegas	58.1	-1.7	1.3
Minneapolis	59.5	0.5	1.3
Model 3	32.6	-.6	.7
New Hampshire	7.4	-.3	.5
Richmond	57.9	-.9	1.2
Model 4	54.2	.6	.9
Tampa	50.2	1.1	1.4
Spokane/Couer d'Alene	58.1	.1	1.1

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes :

Dallas: 570 in the treatment group, 577 in the control group, 1,147 in total
Fort Worth: 386 in the treatment group, 366 in the control group, 752 in total
Phoenix/Las Vegas: 545 in the treatment group 555 in the control group,1,100 in total
Minneapolis: 543 in the treatment group, 469 in the control group, 1,012 in total
New Hampshire: 545 in the treatment group, 538 in the control group, 1,083 in total
Richmond: 542 in the treatment group, 589 in the control group, 1,131 in total
Tampa: 564 in the treatment group, 515 in the control group, 1,079 in total
Spokane/Couer d'Alene: 465 in the treatment group,479 in the control group, 944 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.7
Impacts on SSI Benefits, by Site and Program Model

Average Monthly SSI Benefits, Months 1-30	Control Group	Impact	Standard Error
Model 1	$96	$2	$3
Dallas	95	4	5
Fort Worth	97	1	5
Model 2	$115	$-1	$3
Phoenix/Las Vegas	106	1	5
Minneapolis	124	-2	5
Model 3	$96	$-2	$3
New Hampshire	85	-8*	5
Richmond	107	4	5
Model 4	$136	$-2	$4
Tampa	108	-2	5
Spokane/Couer d'Alene	164	-3	6

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Dallas: 570 in the treatment group, 577 in the control group, 1,147 in total
Fort Worth: 386 in the treatment group, 366 in the control group, 752 in total
Phoenix/Las Vegas: 545 in the treatment group 555 in the control group,1,100 in total
Minneapolis: 543 in the treatment group, 469 in the control group, 1,012 in total
New Hampshire: 545 in the treatment group, 538 in the control group, 1,083 in total
Richmond: 542 in the treatment group, 589 in the control group, 1,131 in total
Tampa: 564 in the treatment group, 515 in the control group, 1,079 in total
Spokane/Couer d'Alene: 465 in the treatment group,479 in the control group, 944 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.8
Impacts on SSDI Benefits, by Site and Program Model

Average Monthly SSDI Benefits, Months 1-42	Control Group	Impact	Standard Error
Model 1	$354	$-10	$9
Dallas	356	-14	12
Fort Worth	352	-7	15
Model 2	$364	$-8	$8
Phoenix/Las Vegas	366	-20*	12
Minneapolis	361	4	12
Model 3	$197	$-3	$7
New Hampshire	50	-3	7
Richmond	343	-3	11
Model 4	$318	$9	$8
Tampa	290	12	12
Spokane/Couer d'Alene	346	6	11

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Dallas: 570 in the treatment group, 577 in the control group, 1,147 in total
Fort Worth: 386 in the treatment group, 366 in the control group, 752 in total
Phoenix/Las Vegas: 545 in the treatment group 555 in the control group,1,100 in total
Minneapolis: 543 in the treatment group, 469 in the control group, 1,012 in total
New Hampshire: 545 in the treatment group, 538 in the control group, 1,083 in total
Richmond: 542 in the treatment group, 589 in the control group, 1,131 in total
Tampa: 564 in the treatment group, 515 in the control group, 1,079 in total
Spokane/Couer d'Alene: 465 in the treatment group,479 in the control group, 944 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.9
Impacts on Benefit Receipt, by Primary Impairment

	Mental			Neurological			Musculoskeletal			Other		
Follow-up Period	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error
					Percentage of months receiving SSI							
Months 1-12	45.7	.1	.6	36.8	-.2	1.8	23.9	-.7	1.0	32.2	.2	.6
Months 13-24	43.0	-.0	.9	33.7	2.5	2.3	23.8	-2.1	1.5	29.8	.2	.9
Months 25-30	42.2	-.4	1.0	32.5	3.4	2.7	22.6	-1.3	1.7	28.6	.1	.9
Months 1-30	43.9	-.1	.7	34.6	1.7	2.0	23.6	-1.4	1.2	30.5	.2	.7
					Percentage of months receiving SSDI.							
Months 1-12	52.8	*-.2*	.4	55.6	-1.6	1.2	52.7	*-.9*	.8	52.4	-.3	.5
Months 13-24	52.1	*-.2*	.6	55.6	-.6	1.8	52.3	*-1.6*	1.2	50.0	-.5	.9
Months 25-30	50.5	*.6*	.8	53.2	.8	2.2	51.1	*-3.0**	1.6	47.5	-.8	1.0
Months 31-42	48.6	*.8*	.8	51.2	2.4	2.3	49.6	*-3.2**	1.8	45.4	-1.6	1.1
Months 1-42	51.1	*.2*	.6	54.0	.2	1.7	51.5	*-2.1**	1.2	49.0	-.8	.8
					Average monthly SSI benefits							
Months 1-12	$137	$2	$2	$110	$6	$6	$77	$-4	$4	$109	$-3	$2
Months 13-24	128	1	3	97	12	9	72	-5	5	100	-4	3
Months 25-30	123	-0	4	95	13	10	70	-8	6	96	-6*	4
Months 1-30	131	1	3	102	10	7	74	-5	4	103	-4	3
					Average monthly SSDI benefits							
Months 1-12	$294	$-1	$3	$345	$-8	$9	$339	$-6	$6	$347	$-5	*$4*
Months 13-24	291	-1	4	344	-4	11	338	-10	9	330	-6	*6*
Months 25-30	282	2	5	324	12	15	328	-14	12	310	-5	*8*
Months 31-42	274	-1	6	314	26	22	317	-16	15	294	-7	*9*
Months 1-42	287	4	5	330	11	20	329	-8	12	321	-3	*7*

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
Mental disabilities: 1,762 in the treatment group, 1,735 in the control group, 3,497 in total
Neurological disabilities: 252 in the treatment group, 223 in the control group, 475 in total
Muscular disabilities: 534 in the treatment group, 486 in the control group, 1,020 in total
Other disabilities: 1,612 in the treatment group, 1,644 in the control group, 3,256 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.10
Impacts on Benefit Receipt, by Gender

	Women			Men		
Follow-up Period	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error
Percentage of months receiving SSI						
Months 1-12	43.5	-.2	.6	32.4	.2	.5
Months 13-24	42.4	-.9	.9	29.5	.5	.7
Months 25-30	41.3	-.3	1.0	28.4	-.1	.8
Months 1-30	42.6	-.5	.7	30.4	.3	.6
Percentage of months receiving SSDI.						
Months 1-12	49.1	-.4	.4	55.6	-.4	.4
Months 13-24	48.8	-.9	.6	53.5	-.4	.7
Months 25-30	47.7	-1.2	.8	51.0	0.0	.8
Months 31-42	46.3	-1.9**	.9	48.6	.3	.9
Months 1-42	48.0	-1.1*	.6	52.3	- .1	.6
Average monthly SSI benefits						
Months 1-12	$140	$-3	$2	$100	$1	*$2*
Months 13-24	134	-4	3	90	1	*3*
Months 25-30	131	-6	4	86	-2	*3*
Months 1-30	136	-4	3	93	0	*2*
Average monthly SSDI benefits						
Months 1-12	$260	$-2	$2	$369	$-4	*$3*
Months 13-24	260	-5	4	356	-4	*5*
Months 25-30	253	-6	4	337	-1	*6*
Months 31-42	247	-11*	6	322	1	*7*
Months 1-42	255	-6	5	347	-2	*6*

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes :

Men : 2,398 in the treatment group, 2,364 in the control group, 4,762 in total
Women: 1,762 in the treatment group, 1,724 in the control group, 3,486 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.11
Impacts on Benefit Receipt, by Age at Random Assignment

	Age 18-30			Age 31-45			Age 46 and older		
Follow-up Period	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error
Percentage of months receiving SSI									
Months 1-12	55.7	.5	.9	34.9	-.1	.6	27.7	-.1	.7
Months 13-24	52.9	.3	1.3	32.7	-.4	.8	25.8	.2	.9
Months 25-30	52.3	-1.7	1.5	31.5	.2	.9	24.5	.5	1.0
Months 1-30	53.9	-0.0	1.1	33.3	-.1	.7	26.3	.1	.8
Percentage of months receiving SSDI.									
Months 1-12	37.2	-.3	.6	54.8	-.1	.5	60.5	-.8	.6
Months 13-24	36.2	-.3	.9	53.2	-.5	.7	59.4	-.8	.9
Months 25-30	35.4	-.7	1.1	51.1	-.7	.9	57.1	-.0	1.1
Months 31-42	34.2	-1.3	1.2	48.9	-.9	1.0	55.0	.2	1.2
Months 1-42	35.8	-.7	.8	52.1	-.5	.7	58.1	-.4	.8
Average monthly SSI benefits									
Months 1-12	$185	$-2	$4	$105	$-0	$2	$89	$-2	$2
Months 13-24	171	-2	5	97	-1	3	82	-2	4
Months 25-30	169	-9	6	93	-2	3	77	-1	4
Months 1-30	176	-3	4	100	-1	3	84	-2	3
Average monthly SSDI benefits									
Months 1-12	$176	$-3	$3	$327	$0	$3	$418	$-8*	$5
Months 13-24	169	-1	5	318	-3	5	409	-8	7
Months 25-30	166	-3	6	304	-3	6	391	-3	8
Months 31-42	165	-13*	8	288	-1	7	378	-5	10
Months 1-42	171	-9	6	309	2	6	402	-11	8

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Age 18-30: 925 in the treatment group, 886 in the control group, 1,811 in total.
Age 31-45: 1,893 in the treatment group, 1,909 in the control group, 3,802 in total.
Age 46 and older: 1,342 in the treatment group, 1,293 in the control group, 2,635 in total.

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.12
Impacts on Benefit Receipt, by Length of Time Receiving Benefits

Follow-up Period	Received SSI and/or SSDI less than 3 years			Received SSI and/or SSDI more than 3 years		
	Control Group	Impact	Standard Error	Control Group	Impact	Standard Error
Percentage of months receiving SSI						
Months 1-12	48.4	1.1	.9	47.4	.1	.5
Months 13-24	42.6	1.9	1.2	45.1	-.3	.8
Months 25-30	40.1	1.2	1.3	44.2	.1	.8
Months 1-30	44.4	1.4	1.0	45.8	-.1	.6
Percentage of months receiving SSDI						
Months 1-12	68.6	-1.4**	.7	68.1	.4	.3
Months 13-24	66.2	-2.0**	1.0	66.1	.6	.6
Months 25-30	62.5	-2.1*	1.2	63.8	1.2	.8
Months 31-42	59.1	-1.7	1.3	61.6	.9	.9
Months 1-42	64.3	-1.8*	.9	65.1	.7	.5
Average monthly SSI benefits						
Months 1-12	$146	$3	$3	$147	$-0	*$2*
Months 13-24	125	6	4	138	-1	*3*
Months 25-30	115	1	5	134	-0	*3*
Months 1-30	131	4	4	141	-0	*2*
Average monthly SSDI benefits						
Months 1-12	$414	$-10**	$5	$407	$2	*$2*
Months 13-24	400	-15**	7	395	3	*4*
Months 25-30	377	-16*	8	379	10*	*5*
Months 31-42	356	-14	10	366	8	*7*
Months 1-42	388	-13	8	387	7	*6*

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

< 3 years: 1,328 in the treatment group, 1,295 in the control group, 2,835 in total
> 3 years: 1,891 in the treatment group, 1,864 in the control group, 3,755 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.13
Estimated Impacts on Selected Measures of Health and Well-Being

	Control Group Mean	Impact	(Standard
Overall Health			
Self-reported health excellent or very good	19.3%	-.2.4	(2.1)
Self-reported health improved since date of random assignment	19.2	.4	(2.3)
Functional and Life Skills Limitations			
Has three or more functional limitations[1]	41.8	2.3	(1.8)
Has three or more life skills limitations[2]	26.5	1.9	(2.3)
Alcohol and Drug Use			
Self reported excessive drinker since date of random assignment	11.3	-0.3	(1.8)
Used drugs to get high since the date of random assignment	13.0	2.0	(1.9)
Emotional Problems			
Stayed overnight in a hospital because of emotional problems since date of random assignment	12.9	1.7	(1.9)
Mental Health			
Mental Health Inventory Scale(MHI) score	14.4	-.21	(.29)
Felt sad, blue, or depressed for two weeks or more over the past year	62.6	.3	(2.7)
Better off today than a year ago	59.7	5.0*	(2.9)
Things will be better a year from now	65.8	5.2*	(2.9)
Mini Mental State Examination Scale (MMSE) score	27.2	-.21	(.13)
Work Limitations			
Illness/injury kept respondent in bed at least 7 days during the previous 12 months	40.6	3.1	(2.6)
Health condition prevents work	39.3	-3.9	(2.6)
Health condition prevents full time work	29.6	-2.9	(2.4)
Transportation Problems limit ability to work	40.4	1.1	(2.6)

* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

786 persons in the treatment group, 735 in the control group, 1,521 in total

Source: Follow-up Survey

1 Functional limitations include difficulty seeing words, hearing a conversation, speaking, lifting 10 pounds, walking up a flight of stairs, walking 3 city blocks, or using a telephone.

2 Life skills limitations include getting around inside or outside the home, getting out of bed or out of a chair, taking a bath or a shower, dressing, eating, using the toilet, keeping track of money, preparing meals, or doing light housework

Exhibit D.14
Impacts on Benefit Receipt, by Follow-up Period-- Full Sample
Unadjusted Treatment and Control Group Means

Follow-up Period	Control Group	Treatment Group
	Percentage of months receiving SSI	
Months 1-12	36.7	37.1
Months 13-24	34.5	34.8
Months 25-30	33.5	33.7
Months 1-30	35.2	35.5
	Percentage of months receiving SSDI	
Months 1-12	52.5	52.1
Months 13-24	51.2	50.6
Months 25-30	49.3	48.7
Months 31-42	47.4	46.6
Months 1-42	50.2	49.6
	Average monthly SSI benefits	
Months 1-12	$115	$117
Months 13-24	107	108
Months 25-30	103	102
Months 1-30	109	110
	Average monthly SSDI benefits	
Months 1-12	$321	$319
Months 13-24	313	309
Months 25-30	299	297
Months 31-42	288	284
Months 1-42	306	303

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
4,160 persons in the treatment group, 4,088 in the control group, 8,248 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.15
Impacts on Benefit Receipt, by Title of Eligibility
Unadjusted Treatment and Control Group Means

	SSI		SSDI Only		Both SSI and SSDI		Neither SSI or SSDI		SSI Applicants	
Follow-up Period	Control Group	Treatment Group	Control Group	Treatment Group	Control Group	Treatment Group	Control Group	Treatment Group	Control Group	Treatment Group
					Percentage of months receiving SSI					
Months 1-12	92.0	91.3	2.4	2.9	83.9	85.6	3.4	2.6	3.5	2.2
Months 13-24	84.2	84.0	3.9	3.9	73.4	76.5	6.3	4.5	6.3	4.1
Months 25-30	81.9	81.0	3.7	4.2	69.4	72.5	7.1	4.8	7.1	4.4
Months 1-30	86.9	86.3	3.3	3.5	76.8	79.3	5.3	3.8	5.3	3.4
					Percentage of months receiving SSDI.					
Months 1-12	5.5	5.8	98.3	97.9	99.1	99.2	2.7	1.8	2.3	1.5
Months 13-24	7.3	7.4	94.0	93.0	94.9	96.3	4.5	3.1	4.4	2.9
Months 25-30	7.8	7.8	88.8	88.5	91.9	93.3	6.0	3.7	5.7	3.5
Months 31-42	7.9	8.2	84.9	83.8	87.0	90.1	6.8	3.9	6.7	3.7
Months 1-42	7.0	7.2	91.9	91.1	93.4	94.9	4.8	3.0	4.6	2.8
					Average monthly SSI benefits					
Months 1-12	$361	$360	$3	$5	$134	$138	$12	$7	$12	*$6*
Months 13-24	330	330	5	7	109	117	21	12	22	*12*
Months 25-30	320	317	4	6	94	97	24	14	25	*14*
Months 1-30	340	339	4	6	116	121	18	10	18	*9*
					Average monthly SSDI benefits					
Months 1-12	33	$32	$669	$676	$402	$388	$19	$15	*$16*	*$12*
Months 13-24	42	39	341	644	388	381	29	23	*26*	*20*
Months 25-30	43	40	604	611	378	374	36	25	*32*	*24*
Months 31-42	43	41	579	579	360	362	40	26	*36*	*25*
Months 1-42	40	38	626	630	383	377	30	22	*27*	*20*

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Received SSI at random assignment: 1,096 in the treatment group, 1,064 in the control group, 2,160 in total
Received SSDI at random assignment: 1,570 in the treatment group, 1,556 in the control group, 3,136 in total
Received SSI and SSDI at random assignment: 553 in the treatment group, 539 in the control group, 1,092 in total
Received Neither SSI nor SSDI at random assignment: 941 in the treatment group, 929 in the control group, 1,870 in total
Received Neither SSI nor SSDI at random assignment, SSI applicants: 701 in the treatment group, 712 in the control group, 1,413 total.

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.16
Impacts on Benefit Receipt, by Primary Impairment
Unadjusted Treatment and Control Group Means

	Mental		Neurological		Musculoskeletal		Other	
Follow-up Period	Control Group	Treatment Group	Control Group	Treatment Group	Control Group	Treatment Group	Control Group	Treatment Group
Percentage of months receiving SSI								
Months 1-12	45.4	45.6	33.6	38.6	24.0	22.9	31.6	32.3
Months 13-24	42.9	42.8	30.9	38.1	24.1	21.3	29.3	29.9
Months 25-30	42.0	41.8	30.1	37.5	23.0	20.9	28.1	28.3
Months 1-30	43.7	43.7	31.8	38.2	23.8	21.9	30.0	30.5
Percentage of months receiving SSDI.								
Months 1-12	52.6	52.3	58.6	50.1	51.8	51.8	51.9	52.2
Months 13-24	51.8	51.5	58.1	51.4	51.4	50.6	49.6	49.4
Months 25-30	50.2	50.8	55.8	50.2	50.6	48.1	47.1	46.4
Months 31-42	48.2	49.1	54.1	49.7	49.0	46.5	45.1	43.5
Months 1-42	50.8	50.9	56.8	50.4	50.7	49.4	48.6	48.1
Average monthly SSI benefits								
Months 1-12	$136	$138	$98	$126	$75	$74	$107	*$106*
Months 13-24	127	129	85	118	71	68	99	*96*
Months 25-30	122	123	83	116	70	63	94	*89*
Months 1-30	130	131	90	121	72	70	101	*99*
Average monthly SSDI benefits								
Months 1-12	$295	$289	$356	318	$327	$334	$341	*$346*
Months 13-24	292	285	352	322	327	329	325	*326*
Months 25-30	283	280	332	318	320	315	306	*305*
Months 31-42	272	271	328	317	311	302	292	*286*
Months 1-42	286	282	343	319	322	321	318	*317*

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
Mental disabilities: 1,762 in the treatment group, 1,735 in the control group, 3,497 in total
Neurological disabilities: 252 in the treatment group, 223 in the control group, 475 in total
Muscular disabilities: 534 in the treatment group, 486 in the control group, 1,020 in total
Other disabilities: 1,612 in the treatment group, 1,644 in the control group, 3,256 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.17
Impacts on Benefit Receipt, by Gender
Unadjusted Control and Treatment Group Means

Follow-up Period	Women		Men	
	Control Group	Treatment Group	Control Group	Treatment Group
Percentage of months receiving SSI				
Months 1-12	42.7	43.5	32.3	32.4
Months 13-24	41.7	41.7	29.3	29.7
Months 25-30	40.7	41.3	28.3	28.1
Months 1-30	41.9	42.3	30.3	30.4
Percentage of months receiving SSDI.				
Months 1-12	48.5	48.6	55.4	54.6
Months 13-24	48.2	47.9	53.4	52.5
Months 25-30	47.0	46.4	51.0	50.4
Months 31-42	45.7	44.3	48.6	48.3
Months 1-42	47.4	46.9	52.2	51.6
Average monthly SSI benefits				
Months 1-12	$138	$137	$99	*$102*
Months 13-24	131	130	89	*91*
Months 25-30	128	125	84	*84*
Months 1-30	134	132	92	*94*
Average monthly SSDI benefits				
Months 1-12	$259	$258	$366	*$363*
Months 13-24	258	255	353	*349*
Months 25-30	250	246	335	*334*
Months 31-42	244	235	320	*319*
Months 1-42	253	249	345	*342*

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes :

Men : 2,398 in the treatment group, 2,364 in the control group, 4,762 in total
Women: 1,762 in the treatment group, 1,724 in the control group, 3,486 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.18
Impacts on Benefit Receipt, by Age at Random Assignment
Unadjusted Control and Treatment Group Means

Follow-up Period	Age 18-30 Control Group	Age 18-30 Treatment Group	Age 31-45 Control Group	Age 31-45 Treatment Group	Age 46 and older Control Group	Age 46 and older Treatment Group
			Percentage of months receiving SSI			
Months 1-12	54.1	57.4	34.8	34.2	27.5	27.1
Months 13-24	51.4	54.3	32.7	31.7	25.6	25.7
Months 25-30	51.0	51.9	31.7	31.0	24.3	24.9
Months 1-30	52.4	55.1	33.4	32.5	26.1	26.1
			Percentage of months receiving SSDI.			
Months 1-12	38.7	35.1	54.5	54.5	59.0	60.3
Months 13-24	37.4	34.2	53.0	52.5	58.0	59.1
Months 25-30	36.6	33.0	50.9	50.2	55.6	57.5
Months 31-42	35.5	31.5	48.7	47.7	53.6	55.6
Months 1-42	37.1	33.5	51.9	51.4	56.7	58.2
			Average monthly SSI benefits			
Months 1-12	$179	$187	$103	$105	$90	$84
Months 13-24	166	175	96	96	82	77
Months 25-30	164	166	92	90	77	74
Months 1-30	171	178	98	99	84	79
			Average monthly SSDI benefits			
Months 1-12	$185	$161	$322	$331	$412	$410
Months 13-24	177	157	314	318	404	401
Months 25-30	174	152	299	303	385	388
Months 31-42	171	144	286	287	370	374
Months 1-42	177	154	306	311	394	394

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:

Age 18-30: 925 in the treatment group, 886 in the control group, 1,811 in total.
Age 31-45: 1,893 in the treatment group, 1,909 in the control group, 3,802 in total.
Age 46 and older: 1,342 in the treatment group, 1,293 in the control group, 2,635 in total.

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Exhibit D.19
Impacts on Benefit Receipt, by Length of Time Receiving Benefits
Unadjusted Treatment and Control Group Means

	Received SSI and/or SSDI less than 3 years		Received SSI and/or SSDI more than 3 years	
Follow-up Period	Control Group	Treatment Group	Control Group	Treatment Group
	Percentage of months receiving SSI			
Months 1-12	45.6	46.6	47.1	47.6
Months 13-24	40.0	41.6	44.8	45.0
Months 25-30	37.4	38.7	43.9	44.4
Months 1-30	41.8	43.0	45.5	45.9
	Percentage of months receiving SSDI			
Months 1-12	66.3	64.2	67.8	68.5
Months 13-24	63.6	61.2	65.8	66.7
Months 25-30	59.9	57.3	63.5	65.1
Months 31-42	56.3	54.3	61.4	62.6
Months 1-42	61.8	59.5	64.8	65.8
	Average monthly SSI benefits			
Months 1-12	$144	$152	$147	$147
Months 13-24	123	132	139	138
Months 25-30	114	119	134	134
Months 1-30	130	137	141	141
	Average monthly SSDI benefits			
Months 1-12	$416	$405	$404	*409*
Months 13-24	401	385	393	*398*
Months 25-30	376	358	377	*389*
Months 31-42	354	338	366	*374*
Months 1-42	388	373	386	*393*

n.s. not statistically significant at the 10 percent level
* Statistically significant at the 10 percent level
** Statistically significant at the 5 percent level
*** Statistically significant at the 1 percent level

Sample sizes:
< 3 years: 1,328 in the treatment group, 1,295 in the control group, 2,835 in total
> 3 years: 1,891 in the treatment group, 1,864 in the control group, 3,755 in total

Administrative data on benefit receipt were provided by SSA/ORES from the MBR810/811 and SSR831 source files.

Appendix E
Cost-Effectiveness Analysis Methods

This appendix provides details on the methods used to estimate demonstration costs and benefits in Chapter 6. It also includes exhibits that display detailed results of the cost-effectiveness analysis for the four treatment models.

E.1 Cost Estimation Methods

In estimating program costs for the cost-effectiveness analysis, the goal was to identify the dollar value of all resources used in delivering the Project NetWork program treatment. We were interested in estimating the ***net*** cost of the program, just as we were interested in estimating the program's net impacts on earnings, SSI, and other outcomes in the impact analysis presented in other chapters of the report. Thus, the cost estimation effort involved a comparison of the cost of Project NetWork and other services received by the treatment group to the cost of services received by members of the control group. The net cost estimates yielded by this effort indicate the costs of all services that members of the treatment group received – whether or not they were paid for by the Project NetWork demonstration – over and above what would have been spent on them in the absence of the demonstration.

Both the direct and indirect costs of Project NetWork were estimated. The direct costs were borne by the Social Security Administration to operate the demonstration program. These expenditures, which cover purchased services, site operations, and program administration, were only incurred for the treatment group. The control group could not participate in NetWork, so none of the direct costs were associated with control group members.

The indirect costs – borne by state vocational rehabilitation agencies, JTPA agencies, and other organizations – were incurred for both the treatment and the control groups. In some instances treatment group members obtained these services on their own. In other cases the Project NetWork program staff referred treatment group members to the agencies offering the services, or even arranged the services themselves; the costs of the services, however, were not reimbursed by Project NetWork. Indeed, in the Referral Manager model (Model 4) such arrangements were an explicit part of the program model. The control group, of course, was eligible to receive all of these non-NetWork services. Therefore, these indirect costs were measured for treatment and control group members alike.

E.1.1 Calculating Project NetWork's Direct Costs per Treatment Group Member

As indicated in the first panel of Exhibit E.1, the cost of Project NetWork-purchased services was estimated using automated records of purchased services.[1] A full accounting of all payments for purchased services was available for the demonstration in these data. As discussed in the text, the services fall into four categories: assessment services; employment and training services; medical treatment services; and other services. To estimate the cost of each category of service, two types of information were necessary: (1) the percentage of the treatment group that received that service, and (2) the unit cost of the service, which is the average cost of serving one person. Both types of information came from the program records data. Multiplying the unit cost by the proportion of the treatment group that received the service yielded the average cost of purchased services per treatment group member.

The cost of Project NetWork's site operations and central administration were estimated separately (see second panel of Exhibit E.1). In this case we needed to know (1) the average length of participation in Project NetWork by the treatment group, and (2) the costs of site operations and central administration per person-month. The average length of participation was estimated as the average number of months between intake and the end of program operations at a given site. Average costs per person-month were estimated using administrative cost data for the central office and the eight demonstration sites maintained by the Office of Disability.

E.1.2 Calculating Non-NetWork Costs per Treatment Group Member

Non-NetWork services included physical therapy, counseling, training and life skills, assessment, occupational therapy, job search assistance, business skills training, college classes, other job-related training, and other services. To estimate the cost of each type of service, we determined (1) the proportions of the treatment group and the control group that received the service, and (2) the unit cost of the service. Multiplying the unit cost by the proportion that received the service yielded the average cost of non-NetWork services for that group.

[1] The demonstration sites used the Case Management Control System (CMCS), or its equivalent in Richmond and Phoenix, to record purchased services. Purchased services were tracked in the 347log file component of the CMCS.

Exhibit E.1
Summary Of Estimation Methods Used In The Cost-Effectiveness Analysis

Component	Behavioral Variable X	*Dollar Value* =	Value Of Component
	Project Network Purchased Services		
Purchased Services: Assessment	Combined NetWork service receipt rate for medical, psychological, and vocational assessments [*program data]*	Cost of purchased assessments per participant	Cost of purchased NetWork assessments per treatment group member
Purchased Services: Employment and Training	NetWork service receipt rate for employment and training services[*program data]*	Cost of purchased employment and training services per participant	Cost of purchased NetWork employment and training services per treatment group member
Purchased Services: Medical Treatment	Combined NetWork service receipt rate for medical, psychological, and drug and alcohol treatments *[program data]*	Cost of purchased treatments per participant	Cost of purchased NetWork medical treatments per treatment group member
Purchased Services: Other	NetWork service receipt rate for other services[*program data]*	Cost of purchased other services per participant	Cost of other purchased NetWork services per treatment group member
	Project Network Site Operations/Central Administration		
NetWork Site Operations	Months of NetWork program participation *[intake and site operations dates]*	Cost of NetWork site operations per month per *person [program data]*	Cost of NetWork site operations per treatment group member
NetWork Central Administration	Months of NetWork program participation [*intake and site operations dates]*	Cost of NetWork central administration per month per person *[program data]*	Cost of NetWork central administration per treatment group member
	Non-NetWork Services		
Non-NetWork Services: Physical Therapy	Treatment-control difference in receipt of non-NetWork physical therapy services *[survey data]*	Cost of physical therapy per state VR service recipient in 1994 *[VR cost and caseload data]*	Non-NetWork physical therapy cost saving per treatment group member
Non-NetWork Services: Counseling	Treatment-control difference in receipt of non-NetWork counseling services *[survey data]*	Cost of other services per state VR service recipient in 1994 *[VR cost and caseload data]*	Non-NetWork counseling cost saving per treatment group member
Non-NetWork Services: Training and Life Skills	Treatment-control difference in receipt of non-NetWork training and life skills services [*survey data]*	Cost of training services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork training cost saving per treatment group member
Non-NetWork Services: Assessment	Treatment-control difference in receipt of non-NetWork assessment services *[survey data]*	Cost of diagnostic services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork assessment cost saving per treatment group member

Exhibit E.1 – *Continued*
Summary Of Estimation Methods Used In The Cost-Effectiveness Analysis

Component	Behavioral Variable X	*Dollar Value* =	Value Of Component
Non-NetWork Services: Occupational Therapy	Treatment-control difference in receipt of non-NetWork OT services *[survey data]*	Cost of physical therapy services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork OT cost saving per treatment group member
Non-NetWork Services: Job Search Assistance	Treatment-control difference in receipt of non-NetWork JSA services *[survey data]*	Cost of training services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork training cost saving per treatment group member
Non-NetWork Services: Business Skills Training	Treatment-control difference in receipt of non-NetWork skills training services *[survey data]*	Cost of training services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork training cost saving per treatment group member
Non-NetWork Services: College Classes	Treatment-control difference in receipt of non-NetWork college classes [survey data]	Cost of post-secondary training services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork college class cost saving per treatment group member
Non-NetWork Services: Other Job-Related Training	Treatment-control difference in receipt of other non-NetWork job training services [survey data]	Cost of training services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork training cost saving per treatment group member
Non-NetWork Services: Other Services	Treatment-control difference in receipt of other non-NetWork services [survey data]	Cost of other services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork other services cost saving per treatment group member
Non-NetWork Services: Other Assistance	Treatment-control difference in receipt of other non-NetWork assistance [survey data]	Cost of eligibility and maintenance services per state VR service recipient in 1994 [VR cost and caseload data]	Non-NetWork assistance cost saving per treatment group member
	SSI and SSDI benefits		
SSI and SSDI	Treatment-control difference in SSI and SSDI payments during observation period *[administrative records data]*	1 / (1 + discount rate)	Change in SSI and SSDI payments per treatment group member
SSI and SSDI Administration	Treatment-control difference in months receiving SSI or SSDI during observation period *[administrative records]*	Administrative cost per month receiving SSI and SSDI in 1994 *[federal data]*	Change in SSI and SSDI administration cost per treatment group member
	Other Transfer Benefits		
Food Stamps	Treatment-control difference in value of food stamps received in month prior to survey *[survey data]*	(1 / (1 + discount rate)) x 15	Change in food stamps per treatment group member

Exhibit E.1 - *Continued*
Summary Of Estimation Methods Used In The Cost-Effectiveness Analysis

Component	Behavioral Variable X	*Dollar Value* =	Value Of Component
Food Stamps Administration	Treatment-control difference in food stamps receipt in month prior to survey *[survey data]*	Administrative cost per month receiving food stamps in 1994 x 15 *[federal data]*	Change in food stamps administrative cost per treatment group member
Medicaid	Treatment-control difference in Medicaid receipt in month prior to survey *[survey data]*	Average cost of Medicaid payments in 1994 x 1.25 *[federal data]*	Change in Medicaid payments per treatment group member
Medicaid Administration	Treatment-control difference in Medicaid receipt in month prior to survey *[survey data]*	Administrative cost per Medicaid recipient in 1994 x 1.25 *[federal data]*	Change in Medicaid administrative cost per treatment group member
	Earnings, Fringe Benefits, Taxes, Unemployment Insurance		
Earnings	Treatment-control difference in earnings *[administrative records data]*	1 / (1 + discount rate)	Change in earnings per treatment group member
Fringe Benefits	Treatment-control difference in earnings *[administrative records data]*	Fringe benefit rate / (1 + discount rate) *[federal data]*	Change in fringe benefits per treatment group member
Federal Income Tax	Treatment-control difference in total earnings *[administrative records]*	(EITC subsidy rate x .4) - lowest federal tax rate x .6) *[federal tax rules, survey data]*	Change in federal income taxes per treatment group member
State Income Tax	Treatment-control difference in earnings *[administrative records data]*	Average state tax rate on income below $10,000 *[state tax rules]*	Change in state income taxes per treatment group member
Social Security Tax	Treatment-control difference in earnings[*administrative records data]*	Social Security tax rate *[federal tax rules]*	Change in Social Security taxes per treatment group member
Sales Taxes	Treatment-control difference in earnings, SSI payments, and SSDI payments *[administrative records data]*	Percent of consumption on taxable items x sales tax rate *[federal data, state tax rules]*	Change in sales taxes per treatment group member
Unemployment Insurance	Treatment-control difference in UI payments received in month prior to survey *[survey data]*	(1 / (1 + discount rate)) x 15	Increased UI receipt per treatment group member
UI Administration	Treatment-control difference in UI receipt in month prior to survey *[survey data]*	Administrative cost per UI recipient in 1994 x 1.25 *[federal data]*	Change in UI administrative cost per treatment group member

As indicated in the third panel of Exhibit E.1, the follow-up survey was used to measure non-NetWork service use. In the survey, respondents in the treatment and control groups reported

whether or not they received each type of service and, if they did, whether the service came from NetWork. For the treatment group, if an individual reported receiving a service and said it was ***not*** provided by Project NetWork, this was counted as a non-NetWork service. If the person said the service was provided by Project NetWork, or didn't know whether it was, we didn't count it as a non-NetWork service, assuming the service had already been counted as a Project NetWork purchased service (see discussion in previous section).

If a respondent in the control group reported receiving a service it was counted as a non-NetWork service even if the person said Project NetWork provided it. A self-report by a control group member that Project NetWork had provided the service in question was assumed to be an error.

The unit costs of non-NetWork services were derived by dividing (1) the total cost of each type of service for state vocational rehabilitation agencies in 1994 in the states where the demonstration sites operated, by (2) the number of clients who received that service during the year. The data were obtained from the Rehabilitation Services Administration of the U.S. Department of Education. The direct and indirect cost of Project NetWork per treatment group member at each demonstration site were estimated using these methods. The site-specific cost estimates are discussed below.

E.2 Benefit Estimation Methods

All estimates of Project NetWork's benefits are based on the administrative records data and survey data described in Appendix A. Program effects were measured as regression-adjusted treatment-control differences in means of outcome measures, such as earnings and Medicaid participation. For outcomes denominated in dollars, such as earnings, program effects were discounted to reflect their present value in 1994, the base year for this analysis. For other outcomes, such as Medicaid participation, program effects were multiplied by a dollar value, expressed in 1994 dollars.

E.2.1 Earnings, SSI Payments, and SSDI Payments

As indicated in the fourth and sixth panels of Exhibit E.1, earnings, SSI payments, and SSDI payments were estimated as regression-adjusted treatment-control differences using the automated records data described in Appendix A. The estimated differences cover the full observation period for each outcome. For earnings, this period lasted from random assignment through 1996. On average the observation period was three and a half years[2]. For SSI payments, the observation period covered 30 months following random assignment. The observation period for SSDI payments was 42 months.

[2] The three full years of followup on all sample members (1994, 1995, and 1996) are covered by the earnings impact estimates reported in Exhibit 4.1 An additional year (1993) is covered by the estimates used in this analysis. For most sample members, part of this additional year occurred prior to random assignment. However, the treatment-control differences in 1993 represent unbiased impact estimates regardless of when during the year sample members were assigned.

In all three cases, measured treatment-control differences were discounted to reflect their present value in 1994, the base year for this analysis. This was done using a real discount rate of 5 percent.

E.2.2 Fringe Benefits, Tax Payments, and SSI /SSDI Administration

As the Exhibit shows, several other benefits were estimated using treatment-control differences calculated using the same automated records data. Fringe benefits were estimated as the estimated earnings impact times an estimate of fringe benefits as a percent of earnings. This estimated percentage, which includes employer-paid health and life insurance, pension contributions, and workers' compensation, is 15 percent of earnings. [3]

Tax payments were also estimated as a fraction of earnings impacts. The percentage of earnings that results in federal and state tax liability was estimated based on the federal and state tax rules in effect in 1994, including rules for tax credits such as the federal Earned Income Tax Credit. Social Security payroll taxes were estimated using the payroll tax rates in force in 1994. The estimated effect of the program on sales and excise taxes reflects state and local tax rates, treatment-control differences in earnings ***and*** SSI and SSDI payments, and the estimated percentage of income spent on taxable goods and services.[4]

Regression-adjusted treatment-control differences in months of SSDI and SSI benefit receipt were valued using estimates of the administrative costs of the two programs, per recipient month in fiscal year 1994.

E.2.3 Other Benefits

Project NetWork's effects on other transfer program payments and administrative costs were estimated using survey data. Estimates of the program's effects on Food Stamps, Medicaid, and Unemployment cover the month prior to the survey. Impacts over the full period covered by the survey (30 months on average), were estimated by assuming that the impacts (1) were zero at the point of random assignment, and (2) grew at a constant rate from zero to the measured difference 30 months later. Thus, as shown in Exhibit E.1, the impact estimates were multiplied by 15.

E.3 Direct and Indirect Cost Results for Demonstration Sites

Estimates of the direct and indirect cost of Project NetWork by site are shown in Exhibits E.2 and E.3. The analysis was performed using the techniques described in the text and in this appendix, and the results are presented in the same format as the exhibits in the text.

[3] U.S. Bureau of the Census (1995).

[4] Tax rules and rate information were obtained from the *1994 U.S. Master Tax Guide* (Chicago: CCH Inc.) and *State Tax Laws* (Chicago:CCH Inc 1995). Taxable consumption rates were calculated using the Consumer Expenditure Survey (1994-1996), U.S. Department of Labor, Bureau of Labor Statistics.

Exhibit E.2
Direct Costs per Treatment Group Member, By Component and Site

Full Sample	
Purchased Assessment Services	$264
Purchased Employment & Training	625
Purchased Medical Treatment	63
Other Purchased Services	99
Site Operations	2,397
Central Administration	212
Total	$3,660
Dallas	
Purchased Assessment Services	$366
Purchased Employment & Training	651
Purchased Medical Treatment	32
Other Purchased Services	82
Site Operations	2,458
Central Administration	737
Total	$4,326
Fort Worth	
Purchased Assessment Services	$249
Purchased Employment & Training	528
Purchased Medical Treatment	11
Other Purchased Services	12
Site Operations	2,557
Central Administration	737
Total	$4,094
Minneapolis	
Purchased Assessment Services	$169
Purchased Employment & Training	366
Purchased Medical Treatment	17
Other Purchased Services	15
Site Operations	3,321
Central Administration	13
Total	$3,901
Phoenix/Las Vegas	
Purchased Assessment Services	$276
Purchased Employment & Training	637
Purchased Medical Treatment	130
Other Purchased Services	162
Site Operations	2,627
Central Administration	13
Total	$3,845

Exhibit E.2 - *Continued*
Direct Costs per Treatment Group Member, By Component and Site

New Hampshire	
Purchased Assessment Services	$222
Purchased Employment & Training	836
Purchased Medical Treatment	48
Other Purchased Services	78
Site Operations	2,296
Central Administration	29
Total	$3,509
Richmond	
Purchased Assessment Services	$319
Purchased Employment & Training	1,229
Purchased Medical Treatment	196
Other Purchased Services	294
Site Operations	3,238
Central Administration	29
Total	$5,305
Spokane/ Coeur d'Alene	
Purchased Assessment Services	$321
Purchased Employment & Training	382
Purchased Medical Treatment	14
Other Purchased Services	71
Site Operations	1,263
Central Administration	129
Total	$2,180
Tampa	
Purchased Assessment Services	$76
Purchased Employment & Training	434
Purchased Medical Treatment	159
Other Purchased Services	17
Site Operations	1,314
Central Administration	129
Total	$2,129

Exhibit E.3
Indirect costs per Treatment Group Member, By Type of Service and Site

Type of Service	Average Indirect Cost per Treatment Group Member	Average Indirect Cost per Control Group Member	Net Indirect Cost per Treatment Group Member
	Full Sample		
Physical Therapy	$24	$334	-$310
Counseling	44	568	-524
Training and Life Skills	19	95	-76
Assessment	27	50	-23
Occupational Therapy	9	71	-62
Job Search Assistance	70	146	-76
Business Skills Training	59	88	-29
College Classes	16	80	-64
Other Job-Related Training	31	142	-111
Other Services	8	23	-15
Other Assistance	0	194	-194
Total	$307	$1,791	-$1,484
	Dallas		
Physical Therapy	$40	$367	$-327
Counseling	8	546	-538
Training and Life Skills	8	144	-136
Assessment	13	51	-38
Occupational Therapy	27	59	-32
Job Assistance	3	125	-122
Business Skills Training	0	46	-46
College Classes	21	86	-65
Other Job-Related Training	25	130	-105
Other Services	9	9	0
Other Assistance	0	194	-194
Total	$154	$1,757	$-1,603
	Fort Worth		
Physical Therapy	$0	$162	$-162
Counseling	59	508	-449
Training and Life Skills	0	47	-47
Assessment	18	50	-32
Occupational Therapy	0	0	0
Job Search Assistance	21	116	-94
Business Skills Training	49	87	-38
College Classes	0	70	-70
Other Job-Related Training	49	182	-133
Other Services	21	21	0
Other Assistance	0	194	-194

Total	$217	$1,437	$-1,221

Exhibit E.3 – *Continued*
Indirect costs per Treatment Group Member, By Type of Service and Program Model

Type of Service	Average Indirect Cost per Treatment Group Member	Average Indirect Cost per Control Group Member	Net Indirect Cost per Treatment Group Member
	Minneapolis		
Physical Therapy	$12	$496	$-484
Counseling	37	755	-718
Training and Life Skills	49	211	-162
Assessment	22	37	-15
Occupational Therapy	12	186	-174
Job Search Assistance	63	163	-100
Business Skills Training	57	63	-6
College Classes	13	116	-103
Other Job-Related Training	40	191	-150
Other Services	0	52	-52
Other Assistance	0	194	-194
Total	$305	$2,464	$-2,159
	Phoenix/Las Vegas		
Physical Therapy	$29	$204	$-175
Counseling	29	508	-479
Training and Life Skills	14	49	-35
Assessment	31	65	-34
Occupational Therapy	0	40	-40
Job Search Assistance	41	149	-108
Business Skills Training	62	95	-33
College Classes	8	86	-78
Other Job-Related Training	24	80	-56
Other Services	0	0	0
Other Assistance	0	194	-194
Total	$238	$1,470	$-1,232
	New Hampshire		
Physical Therapy	$22	$349	$-327
Counseling	131	632	-501
Training and Life Skills	36	122	-86
Assessment	30	64	-34
Occupational Therapy	0	41	-41
Job Search Assistance	113	243	-130
Business Skills Training	59	104	-30
College Classes	12	89	-77
Other Job-Related Training	14	260	-246
Other Services	0	11	-11
Other Assistance	0	194	-194
Total	$417	$2,109	$-1,692

Exhibit E.3 – *Continued*
Indirect costs per Treatment Group Member, By Type of Service and Program Model

Type of Service	Average Indirect Cost per Treatment Group Member	Average Indirect Cost per Control Group Member	Net Indirect Cost per Treatment Group Member
	Richmond		
Physical Therapy	$56	$427	$-371
Counseling	37	503	-466
Training and Life Skills	12	46	-34
Assessment	26	33	-8
Occupational Therapy	0	94	-94
Job Search Assistance	62	110	-49
Business Skills Training	38	57	-20
College Classes	10	30	-20
Other Job-Related Training	31	73	-42
Other Services	26	40	-14
Other Assistance	0	194	-194
Total	$298	$1,609	$-1,312
	Spokane/Coeur d'Alene		
Physical Therapy	$12	$245	$-233
Counseling	49	492	-453
Training and Life Skills	37	31	6
Assessment	24	33	-7
Occupational Therapy	24	62	-38
Job Search Assistance	167	149	18
Business Skills Training	135	109	26
College Classes	43	86	-43
Other Job-Related Training	32	99	-67
Other Services	5	10	-5
Other Assistance	0	194	-194
Total	$528	$1,510	$-982
	Tampa		
Physical Therapy	$23	$422	-$399
Counseling	59	658	-599
Training and Life Skills	23	21	2
Assessment	73	92	-19
Occupational Therapy	0	33	-33
Job Search Assistance	69	134	-65
Business Skills Training	83	189	-106
College Classes	24	22	2
Other Job-Related Training	29	152	-123
Other Services	10	71	-61
Other Assistance	0	194	-194
Total	$393	$1,988	-$1,595